

ANTHROPOLOGICAL QUARTERLY

VOLUME 32

JANUARY 1959

NUMBER 1

REDFIELD COMMEMORATIVE ISSUE

Social Anthropology and the Nature of Man

Papers presented at the

INSTITUTE OF SOCIAL ANTHROPOLOGY

ALLEN SPITZER, *Director*

Saint Louis University — June 9-14, 1958

Robert Redfield 1897-1958.....ALLEN SPITZER

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The Catholic University of America Press
WASHINGTON 17, D. C.

(Second Printing -- March 1967)

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The ANTHROPOLOGICAL QUARTERLY is edited by the Department of Anthropology, The Catholic University of America.

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Published quarterly, January, April, July, and October, by the Catholic University of America Press, Washington 17, D. C. Subscription price: Yearly \$3.00. Single number 75 cents.





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SOCIAL ANTHROPOLOGY AND THE NATURE OF MAN

With the generous cooperation of Saint Louis University we are publishing in this special issue a group of papers presented at an Institute on Social Anthropology entitled "Social Anthropology and the Nature of Man." The Institute, organized and directed by Allen Spitzer, was held by Saint Louis University on June 9-14, 1958. The late Robert Redfield was a participant and we are privileged to have this opportunity of publishing his contribution to the Institute, — one of the last papers he wrote. Since the other papers which have been selected show the influence of Redfield's ideas, it seems appropriate to dedicate this number of the ANTHROPOLOGICAL QUARTERLY to his memory. This is only a slight acknowledgment of the debt which all of us owe to one of America's most stimulating and thoughtful anthropologists.

R. F. H.

ROBERT REDFIELD 1897-1958

Despite his recent years of illness, the world of scholars was shocked and grieved by the death on October 16, 1958, of Robert Redfield at the age of sixty. In the years before his passing, he had been acknowledged as one of the great anthropologists. First trained as a lawyer, by nature essentially an artist and a poet, he brought to the field which he finally selected as his life work, the richness and the depth of a truly great mind and spirit. To recount the accomplishments of his life would take many pages; here we can consider but few of them.

Born near Chicago on Dec. 4, 1897, Redfield had been associated with the University of Chicago since his own student days, and had been a member of the faculty since 1927. He served as Dean of the Division of Social Sciences from 1934 to 1946 and was Chairman of the Department of Anthropology from 1947 to 1949. In 1953 he was named Robert Maynard Hutchins Distinguished Professor of Anthropology.

From 1930 to 1943 Redfield was Research Associate in charge of ethnological and sociological field work in Yucatan and Guatemala for the Carnegie Institute. In 1944 he was President of the American Anthropological Association. During World War II he served as an adviser to the War Relocation Authority. Later he attended the initial conferences of the United Nations Educational, Scientific and Cultural Organization in Europe. In 1948 he became a Director of the American Council of Race Relations. When in 1951, the Ford Foundation allocated \$75,000 for the study of the relation of international race relations and world peace, Redfield was selected to direct the project. Following this, he was an exchange professor at the University of Paris in 1952. Later he was chosen by the Royal British Anthropological Society to give the Huxley Memorial Lecture, and was here in his own country the recipient of the Viking Fund Medal for his outstanding contribution to anthropology.

ALLEN SPITZER

ANTHROPOLOGICAL UNDERSTANDING OF MAN

ROBERT REDFIELD

In responding to the invitation to say something as to the contribution made by anthropology to our understanding of man, I choose from among anthropological interests and ways of work that part which I take to be central: the study of entire cultures or societies throughout the world. It is the understanding so reached that I consider in the remarks that follow. Understanding of man, but of different kinds, is reached by those anthropologists who study man's biological nature; and those who address themselves to the study of language, tool-making or food-production and economics learn something of man in the aspect of his nature that appears in the activity chosen for particular attention. But here I try to say something about the nature of man as it appears to those of us who look upon the total, traditionally organized life-ways of many a human group, as these occur in bands, tribes, peasant villages, and rural or urban communities, in any part of the world and at any time in history to which we have access for understanding. The unit of consideration is a whole way of life in some group distinguishable from all others, and the question becomes: What do we learn as to the nature of man as many such life-ways are examined and compared?

Every way of work opens paths to understanding, but also shuts us away from something in the reality excluded by the interests and assumptions of that way. Therefore it may be fair, before making any positive assertions as to the nature of man, to mention, not only the advantages, but also some of the limitations which inhere in reaching understanding of mankind by this way of work.

In spite of a growing disposition on the part of anthropologists to become interested in particular Indians, Africans or Melanesians, I think it can still be truly said that the "man" as to which we get some understanding is man as defined by or described in terms of the customs, institutions and ideas that are characteristic of his community or other organized social group. Most of what we say about man is about a man more or less generalized for his tribe or village or neighborhood or even nation. We do not add very much to understanding of particular human

beings, even though we do sometimes get someone to write his life-story, or we analyze some personality through projective tests or other means. The interest in particular human beings is certainly there, in our work, but it is not the central interest; we can say more about man as a great number of solutions to the problems of human living achieved by groups than we can say about the separate confrontations of existence present in each ultimately lonely one of us human individuals.

From this one moves easily to make another assertion about how our understanding is reached. We look from our own position, that of the anthropological investigator, out, to and inside of those other people, over there, doing things and thinking thoughts that we seek to understand. We do not get much of our understanding of man by looking inward into ourselves. Montaigne is not our model. We are extrospective not introspective. But as I say this I am aware of a qualification of this assertion. While it is true that we look at them, not within ourselves, on the other hand, it is that which is within each one of us anthropologists, being human beings too, that reaches out for understanding of those others. We look at those others not as radar sees, or as a camera records, machine-like, with automatic and inhuman dependability; no, we reach into those others with our own human nature, using the sentiments and ideas that we have within us as other human beings. We could not understand those others at all if we did not or could not feel some part of the sentiments and hold the ideas which we come to understand that they feel and hold. We do not observe "behavior," in the sense of events that may be described in physical terms, as motion and velocity. We observe the overt indications—words, gesture, context of situation—from which, using imaginative sympathy, we infer the states of mind and feeling of those others that we study. We are always trying to connect what in them is different from our own ways of thinking and feeling with what, in some things, is the same. So we "project ourselves" into the mind of that other by means of our own humanity, yet we do so always in doubt that our projection truly represents the mind of that other. Each imagined inference as to how that other feels and thinks is challenged by that part of our minds whose business it is to question and to test; we are not content with a first interpretation; we

hold it in suspense while we look for some confirmation or confutation or modification. So, if in some remote land I see for the first time there a widow give vent to loud mourning at her husband's grave, I may first deeply sympathize with her sorrow, knowing in her place I too should grieve. But also I refer to another aspect of my human experience, namely, that often one does in public what one is expected to do whether or not it quite corresponds to one's inner feeling. So I entertain another possible interpretation: the widow does not feel quite as badly as it appears; perhaps she is obeying proprieties, not tragic passions. And I know I have a question to answer to myself through consideration of other facts. But always goes on this mental moving back and forth between a humanity-propelled insight into the mind of another, and a withdrawal outside again, to look coolly and in doubt at what I think that I found in there.

In these days of Freudianism and what is more generally called "depth psychology," another limiting characteristic of our way of work may be mentioned. In looking into the minds of those others we do not look very deep. The subconscious and the darker and inner impulses of human life, whatever they may be, are not our usual subject matter. True it is that some anthropologists are so concerned, but, if so, their language and ideas have not been developed from their own comparative study of societies and cultures; if those deeper depths are included in the reports they bring of the minds of those others, they are reported in terms borrowed from the depth psychologist himself. On the whole we are concerned with understanding the more manifest aspects of man's nature: his work and worship, the local forms of his desires and his goals, pretty much as he himself knows them.

So I return to the assertion that the understanding of man of which I speak is that reached by sympathetic and yet objectively critical learning about the characteristic mental states of many a human group with a tradition behind it and a manifestation of its organized life-solution as apparent to us in what they say and do and in the customs and institutions they exhibit. Ours is a very wide range of instances of such life-solutions, from the very isolated and primitive, through the peasantry and rural peoples, to town- and city-dwellers of modern nations. Also the range in history is wide: we include in our instances of life-ways those

that flourished long ago, as far as first history and then archaeology can make them known to us—much less intimately and completely, surely, than our direct acquaintance with living peoples permits. So we compare not only one contemporary life-way with another, but also the more primitive with the more civilized, and the more ancient with the more recent. In these comparisons we reach some understanding of what has happened to man through all human history. We have some idea, however sketchy, of the human career, thinking now of man now as one thing, one collective experience, from a time when humanity was not yet, through its beginnings very long ago, and through the coming of the civilizations and—it may seem nowadays—the appearance of something like a world civilization. We have not only a variety of groups ways to consider; we have also the total human biography to write—or to write something about.

The anthropologist's understanding of man may be compared to the biologist's understanding of life. We look at the life-ways of groups as the biologist looks at species and genera. They are natural phenomena of one great family of phenomena, alike but different. We look at them in first place with the eye of natural history: there they are, each is such and such; their natures allow for explanation in terms of their habitats, histories, adjustments and internal arrangements. We collect and compare. And then, again I think like the biologist, we remove ourselves from too close a view of the particular instances of our class of phenomena and ask ourselves more general questions: the biologist, as to the general nature of life, its ways of being, its general processes, its course of development; the anthropologist as to corresponding general questions about man in society. It is about some of these more general understandings that I shall speak.

We are, then, scientists, as is the biologist, in our assumption that there is a real world out there accessible to our observation, in our reliance upon observation and inference guided by reason and observation for our conclusions, in our attempts to be objective and critical of our formulations, in our responsibility both to the facts and inferences we alone have made and stand witness for and also to the correcting judgment, over time, of the scientific community. Revelation has no part in our understanding of man. On the other hand we are not quite like the biologists for the

reason that very much more than is true with them, we must use our complex human natures to understand our subject matter, it being also human. While Conrad Lorenz does project part of himself into some of the animals whose behavior he studies, it is a small part of him that he uses in comparison with the large amount of human nature and experience that we use.

So we are a science with close connections with humanistic learning. Especially with respect to that anthropological way of work of which I am speaking—the understanding of traditionally organized life-ways in human groups directly accessible—we are, I think, characteristically conscious of a human whole; that life-way there is a total provision for a sample of humanity; it covers all the needs; it meets what all people must have—a way to persist, a way to see life as meaningful, a way to express a nature all mankind shares—different as these ways may be. The reporting of such a total provision for humanity is not wholly unlike a novel or a biography or a study of an individual's character: it must preserve the wholeness and the uniqueness of the thing. And this we try to do, returning to the whole again and again after we have drawn out from it some aspect, process or problem, that happens particularly to interest us. The analysis, the breaking up of the whole into some fragment that may be related to a corresponding fragment in that same whole or another goes on as the stricter forms of scientific endeavor proceed, but characteristically it does so under the control of the entire life of the group; the part is seen as belonging to that whole.

Up to this point what I have said is probably acceptable to most anthropologists. But now, as I begin to put forward some assertions as to the nature of man that I take to arise out of our kind of work, I think that I shall less and less speak for anthropology, as a consensus of opinion and judgment, and more and more for myself. If the question be asked of many an anthropologist, What is your understanding of man, as a whole, as one kind of being? (and that anthropologist be a student of custom and institution), I rather think that the question will not bring forward an answer ready and formulated. It is not the question that preoccupies my scientific fellows. They are more apt to be thinking of particular aspects of particular groups of men, and differences among such groups rather than generalizations about all mankind

form the major substance of their talk and work. On the other hand, some of them—and more of them more recently than a generation ago—interest themselves in such matters as are to be suggested in my remarks that follow.

I may, however, begin my short list of propositions about man with an assertion that is, however, very familiar and well within the center of usual anthropological understanding. It is said in the simple statement that the ways of mankind are very different. What is done in one place or group shows striking contrast to what is done in others. Here a people think the best possible marriage for one's daughter is with the son of one's father's brother, and over there that is just the wrong thing to do: she should marry the son of one's father's sister, or, possibly, the son of one's father's younger brother, not of his elder brother. Here is a people who feed their warriors on milk and blood; the Chinese used not to drink milk at all; while the orthodox Hindu goes strongly for milk but certainly not for blood. And so on. It has been an emphasized anthropological contribution that man, though he may in some sense be one, is very many. And this has been emphasized by some to the point of declarations that there is no universal nature of mankind, but only the many different human natures appearing in the many different societies and cultures. Recalling disposition of men's minds — whether they be scientists' minds or those of other people — to begin with an assumption that what one's own people do is natural and right, this counterweighting of the ethnocentric distortion by anthropological knowledge is not unexpected and may be seen to be helpful to better understanding of man.

If this proposition as to the diversity of life-solutions in human groups be looked at, however, in the course of its development within anthropology and allied thought, one begins to see that it has taken various turns and followed a course of re-statement or re-emphasis that begins to bring us nearer to our question today: What is the anthropological understanding of all mankind? There is, for one thing, a difference between that statement as to cultural diversity as a mere matter of fact, an observation of difference, of objective interest and perhaps of something appealing to human curiosity, and that statement of diversity which seems to shake a hearer's confidence in the rightness of his own ways.

To illustrate this difference in emphasis we might contrast Herodotus and William Graham Sumner. When Herodotus reported to his fellow Greeks that the Egyptians practiced circumcision and venerated cows, or that the Scythians took scalps and made drinking cups of the skulls of slain enemies, he was telling them of differences; he was not, I think, trying to shake the Greeks out of an ethnocentric complacency. As I read Sumner's *Folkways*, however, I have constantly the feeling that Sumner was a reformer *manqué*, a disillusioned disillusioner. He wants to show, not merely human diversity, but something not quite human perversity: not merely that other people do other things but that other people do things shocking to us. Witness topics he chose for his documentation: child sacrifice, sacril harlotry, incest, infanticide. On the whole, anthropology has been more in the tradition of Herodotus, but there is also a Sumnerian quality in that anthropological teaching which seeks to disturb parochial satisfaction with one's own way of life by thrusting some fairly violent contrast from Melanesia or Africa upon the reader or listener. "Look," we sometimes have seemed to be saying, "those people there find right and good conduct that you, in your self-satisfaction, think immoral and abhorrent."

For a time in the course of anthropological development the prevailing conclusion of theoretical rather than of educational or propagandistic importance was that anything might somewhere be found to be right and good. The foundation of morality appeared to lie only in the judgments of each particular local community. This was the anthropological rejoinder to both a theologically justified morality and that philosophical morality that assumed or declared the presence of moral faculties or inherent moral insights. Westermarck's well-known book was such a rejoinder. Later anthropologists dropped his method of taking a custom here and a custom there, and turned instead to the study of custom and morality as these appeared in the life-ways of particular societies, usually primitive, and studied intensively by direct observation. As anthropologists came to work themselves more intimately into exotic human societies, they came to understand that if the unfamiliar human being does something even shockingly different from what educated Western people generally do, the "rightness" often lay in how the thing done entered into and was further explicable by something in the total

way of life of that people. It was no longer possible to stop with the assertion "Those people think it right to kill their fathers, or practice cannibalism, or marry one's sister." Seen in its full context, there was much more to be said as to why these things were done and thought right and good. The Eskimos who walled up an aged parent in a snowhouse and left him to die, did so because in their hard, migratory life the old person could no longer travel, endangered his close kinsmen by his presence, and perhaps himself endured an almost unbearable existence. Furthermore, good reporters of actual cases of these assisted suicides—for that they were rather than homicides—show the tenderness, even the filial respect, with which the thing was done. Cannibalism, found to be not one custom but many different kinds of customs, showed, in one of its forms, a ritual partaking of the flesh of a slain enemy into which entered, among other elements of feeling and belief, a respect for the valor, one might say the spiritual strength, of that enemy. And when the Hawaiian chiefs were married to their sisters it was with the idea that the purity of their lineage should be maintained and concentrated in offspring also to be chiefs. I suppose the commoner shared with the chieftain the feeling that this practice was for the general good. I am, of course, not trying to represent primitive life as high-minded and supremely ethical; there is much that is cruel and terrible in what people do. I am merely trying to make the point that to the anthropologist the shock of the different began to disappear as it came to be understood that each traditional way of life was a somewhat coherent statement, in thought and action, of a good life. Seen in context, most customs then showed a reasonableness, a fitness with much of the rest of the life, that allowed the outsider more easily to understand and more reluctantly to condemn. At this point in the development of the understanding of differences in group-ways the phrase "cultural relativism" came into use. The basic tenet of cultural relativism is the proposition that the rightness of what is done by another people follows from *their* view of things, not from ours. Adopting the poetic imagery of an American Indian, Ruth Benedict introduced her so well-known book with the words: "In the beginning God gave to every people a cup of clay, and from this cup they drank their life."

There is, however, another turning in the course of anthro-

poloical understanding of the cultural differences among men. The possibility is entertained that in spite of these many differences there are important resemblances. More emphatically put, the question becomes, Are there universal elements present in all human groups? And, put more guardedly, the query is: Are not some things done and thought right much more commonly than are some others? These questions then are directed to what may be always or generally or very frequently true of mankind, no matter how different are the customs of one group from those of another. In the anthropologist's search for understanding of man, this group of questions has, I think, never gone entirely neglected. In the early years of the science it was supposed that the life-ways of all peoples tended, through long time, to pass through similar stages of custom and institution. In Bastian's "elementary ideas"—a sort of stock of basic human conceptions—, in the readiness of Andrew Lang and others to see separated people responding similarly to the puzzles of life so as to produce, independently, similar myths and explanations, are represented early conceptions of the "aliveness" of men in many different times and places. In these early views of the similarities in custom and belief among widely separated peoples the explanation of the similarities was usually found in a supposed property of all human beings—it was by some called "psychic unity"—which of itself caused different people to have similar beliefs or customs, or caused them, over time, to repeat the discoveries and developments that other peoples had also made and attained. The thought then was that human beings had an implanted potentiality that would, in time and appropriate circumstance, declare itself in a similarity or repetition of custom or belief.

Today we are less inclined to see such similarities as arising from an inherent nature of our species so particular in its properties as to bring about particular customs or beliefs. We think of our inherent nature as plastic and, in effect, unknowable for the reason that in all characteristically human behavior or conduct we do not see its effects except as mixed with the effects of experience—"environment." And that experience, for the people living in traditional groups that anthropologists study, is not only different as we go from group to group but is also, in important ways, similar. So I, for one, incline to see notable

similarities among human beings everywhere, along with the differences, and to think of the similarities as brought about by an original nature characteristic of our species and taking its form in life circumstances that, different as they are, are not absolutely different but necessarily similar. It is then possible to see limiting conditions for human nature as developed in traditional groups. One sees requirements of group living that keep our developed human natures from differing utterly from one group to the next. I think of developed human nature as tending toward central tendencies; comparing two very dissimilar groups—say the warlike Comanche and the peaceful and industrious Maya peasant—we see very different customs and strikingly different choices in the virtues that are emphasized, but we also see Comanche warrior and Maya agriculturalist similar with regard to many human sentiments. We would find each proud, and sensitive, and loyal, although what would stir the pride of one or command his loyalty would probably be different from that which would arouse the same sentiment in the other. It is in large part these sentiments, very human and not animal, that people have in common, hard as they are to describe precisely and obscured as they are by the undeniable differences in custom and group character. The resemblances and the differences among peoples are like the two alternative perceptions of the ambiguous drawings psychologists show us: looked at now, it is only differences that appear to us; looked at again, the common human qualities show themselves. I do not suppose that the Jesuit missionaries to the Huron saw similar human qualities in the fortitude with which the Huron met death by torture and in the missionaries' own fortitude in meeting a horrible death, but looked at from the one point of view, Indian and Jesuit appear as displaying the same heroic human quality.

I do not think we can say with any definiteness just what are the circumstances of life in traditional groups that place limits on the variations in custom and character. Some of these circumstances, however, are plain enough and have been stated by one anthropologist or another. Thus it is clear that there must be some restriction on violence within the small group or little community; it could not persist if custom allowed any man or woman to attack or destroy any other. It follows that within

every group it must be thought wrong to kill anyone you choose, wilfully. And it follows that a sense of oughtness or rightness will, in every group, attach to these restrictions on violence. Or, put another way, everywhere some sense of loyalty or obligation to preserve and support others of one's own group will be present, however much violence against outsiders may be approved. It is equally obvious that in every group we shall find the people concerned with gaining a livelihood, whether the fruit be picked easily from a tree and the fish drawn from the sea, or whether uncertain and arduous hunting in the tropical forest fills the stomach, for without a livelihood no group persists. So there are concerns of practical living that are universal by necessity. The universality of the nuclear family is another commonplace; the fact that formerly among the Nayar fathers were not included, or the fact that in Samoa children move easily from one household and nuclear family to another are facts again suggesting the wide range of familial arrangements, while such facts also remind us that no traditional group gets along solely by recruiting new members from outside; they all conventionalize the relations of procreation and sibship into duties of more or less intimate relationship between old and young that again provide a common, indeed a universal, circumstance of group life. Moreover, in nuclear families, however different from one another, are generated many of the qualities that never depart utterly from the central human tendency: personal interdependence, warmth of sentiment for another, associations that support, as significant and important, what is intimately learned early in life from one's cherishing elder or one's older sibling. There are both life circumstances similarly present everywhere and also resulting qualities of human nature similarly present everywhere. The foregoing examples of limiting conditions leading to general similarities in the life-ways of all traditional groups have to do with sentiments, interests, institutions or moral judgments. Examples can also be found as to the way the material and natural world is seen and conceived. Very different as are the conventional views of space, time and cosmological order, I do not suppose there is any group that sees all parts of the apparent daily movement of the sun as equivalent: all peoples take some account of the difference between night and day.

More speculative limiting conditions to the departure of developed human nature from its central tendencies can be found in the possibility that custom itself can become so burdensome as to cause a rebellious human nature to bring its excesses to an end. Elsewhere I have expressed a doubt that the remarkable event in Hawaii wherein at one blow the people broke their ancient and onerous taboos, the priests themselves destroying their temples, is to be interpreted as occurring entirely without the influence of teaching and example from Europeans. Nevertheless, the case does suggest that Kroeber's point about that case is probably correct, qualify it though one perhaps must. It does seem that the elaborate system of sacred restrictions broke down in part because it was too hard to bear. This is the same as saying that a sort of "basic human nature," with appetites and wishes and impulses for freedom, pushed against the structure of custom and overthrew it. The "fatigue" is of people, who, like other people, had appetites, desires, and impulses to enjoy or to move more freely—qualities of their human nature developed in the Hawaiians in spite of and along with the onerous taboos.

It seems to me that it is not possible to think of all customs as equally acceptable to this prevailing central tendency of developed human nature which I am here suggesting to be a reality. It does not seem to me that the varying demands of different customs are matters of complete indifference to the men and women who live by these customs. It rather seems to me that some customs are more easily followed than others. Thus, the custom of eating with the right hand only, or that of bowing as a respectful salutation, seem to me to demand barely more than the acquisition of a habit: once the expected bodily response to a simple class of situations is fixed. But the sacrifice of one's children or the immolation of widows are customs less easily borne. There are many more societies that do not expect mothers to sacrifice their children than there are societies that do. If we knew how such customs were carried out in case after case in societies where they prevail we should find, I believe, inner struggles, protests and occasional refusals or escapes that would show us that such customs ask very much of that "nuclear human nature" which I imagine we may justly conceive. Indeed, the case of suttee in India, where the documentation is fairly full, does show that

although many a widow went with courage and conviction of bliss to her fiery death, others struggled against it and were constrained by the demands or coercions of others. It is this line of thought that directed my interest to the Pawnee chieftain who, singlehanded, twice rescued a captive about to be shot to death by arrows. What moved him to go so boldly against a religious custom of his people? One notes that this was (as I recall it) the the only human sacrifice practiced by the Pawnee, and also that the tribes around them did not practice such a custom. The context of approval or disapproval or indifference within the local group and the wider community is, I imagine, also a factor, along with the demands on human nature, in making a custom more or less easily followed. True though it is that custom can make anything right, I think that there are great differences among these "anythings," some requiring great sanction and support, others not, some easily obeyed, others only imperfectly, and by drawing on the deeper wells of human fortitude, faith and commitment. I think of developed human nature as capable of meeting very exacting demands and yet not capable of meeting all of them equally. Developed human nature is not only a "central tendency"; it is something potentially capable of extraordinary development, but it has its wavering points, its breaking points.

I return again to the question implied by the title given me, the question, *What*, if anything, can the anthropologist be said to have come to understand about man? I take the question to call for some positive assertions about people—in traditional groups—everywhere. What can be said now, not about differences but about prevailing resemblances as to the ways of life in such groups or about how people in such groups generally think and feel?

So understood, the question does not call for a listing of customs or institutions that are universal or nearly so—like the nuclear family and provisions for subsistence—but for assertions referring to widespread or possibly universal mental states—mental states comprehensive of much of life's experiences. I will put forward a few such assertions.

I begin with something that is no fresh discovery of mine but a fact familiar to comparative students of the traditional societies: the people of all such groups look out upon a universe exhibiting

significant order. The orientation of the self and of all the other selves in one's own traditional group to a world seen, not as chaotic but as having arrangement: out of all that is seeable or conceivable certain elements only are, as it were, chosen, and these elements have a fitness to one another and to the interests of men. Put briefly, in every such group there is an intelligible world view. The people of that community find in this view an understandable significance both of things about them and of their own customary actions. Life has a meaning. Elements of what we call "nature"—and elements conceived as existing and significant though perhaps unseen and immaterial are so composed as to document and justify the traditional course of life. So it follows that in these groups there is an authentication of existence, of man's efforts, successes and failures. These words are very general; they are designed to include the great variety of such world-views, some simple, like that of the Andamanese, some complex, like that of the Zuni; some shared about equally by all members of a little-differentiated group—again the Andamanese serve as an example—and some compound, in the sense that lay knowledge of the universe is simpler than the more esoteric knowledge of specialists or of specially instructed persons. Certain West African peoples provide instances of compound world views. But the main point is the universality of meaningful conceptions of the universe and of man's place in it.

Not fully separable from this assertion is another: the view of things of every people includes rightness and "oughtness." What is mixes itself with what ought to be and with what one ought to do. Everywhere there are recognized obligations, commitments, sentiments and judgments of what is good and what is bad. No people is morally indifferent. If we let "morality" stand for all such judgments and commitments as to what it is felt right or wrong to do, not because it is merely prudential or expedient but because it is in itself right and obligatory, then morality is universal. It is again only recognizing the other aspect of the double truth to say that just *what* is moral and *what* is contrary to moral obligation varies very widely: here I am asserting the balancing truth that everywhere *something* is right and *something* is wrong. Again, this assertion is to be read in connection with what has been earlier put forward here as to possible limits as to

what may be approved, and as to differences in the demands made by different rules of conduct.

It is the universality of morality, in this sense, and not of religion, that may with confidence be put forward. From the very wide comparative viewpoint which I am here trying to take, religion appears as one of a number of possible ways in which man may shape his relationship to that which is not himself but is outside him while consequential to him. It is, I suppose, possible so widely to define religion as to allow the assertion that all peoples are religious. If religion be thought of as present wherever an attitude of fearful or respectful attention is given to something, whether personalized being or not, that is out of the ordinary, the workaday, the *ausseralltäglich*, then it may be that all the peoples known to anthropologists "are religious." But such a way of thinking of the matter would tend to make magic and religion indistinguishable, and, further, it would overlook the large differences between peoples who strongly seek to establish with a personalized "other" a relationship of well-being and those who, by ritual or formula predominantly constrain unpredictable events to particular ends in view. Malinowski's Trobrianders appear as magical practitioners; there is little religion, in a narrower sense, among them. When the spirits of the dead return, the occasion is more like a Halloween among us than like a Mass for souls. And the Arapesh, as described by Margaret Mead, have their attentions directed primarily on the mysterious power of their own sexuality and much conduct is governed by the ideas they have as to its management. Their spiritual beings of the bush, the *marselai*, are hobgoblin-like beings; no religious attitudes—again in the narrower sense—attend them. No, what mankind has invariably done, in each traditional group, is to develop more or less orderly ideas about a universe to which, in some way, man is significantly related, or of which he is a part, and to clothe these relationships and significances—in which, of course, those of man to man are invariably included—with feelings of inherent rightness and obligation. The emphasized quality of the relationships to a powerful and unseen something may or may not be stressed, may or may not take the form of propitiation rather than constraint, may or may not be directed toward godlike beings, and may or may not be prudential, or, on the other hand, what we call "spiritual."

I do not here prolong this attempt to characterize with sweeping inclusion mankind as known to anthropologists, except to add one more assertion, in which again the generally human nature of our kind may be suggested. The peoples studied by anthropologists, taken as whole groups acting and thinking over long periods of time, show a creativeness beyond the demands of subsistence and mere survival. In such a group, however meager its resources and however hardpressed it is to survive, we see some production, an accumulation of generations, in which are expressed imagination, a sense of coherence, a progressive building of some "work of the mind." I am thinking of systems of myth or of ritual or both, of complexities of arrangement of the parts and relationships of their own society—as among the Australian aborigines—of the more sophisticated calendrical productions of the ancient Maya, of oral texts or chants of Navaho or of the aboriginal Hawaiians. If one takes him collectively and in persisting traditional communities, man is a poet, a maker of things expressive, of works beyond his material needs. These things he finds significant and as to their renditions or productions, in legend, art, morality, social relationship or wherever these creations occur, he makes discriminations of judgment that are more than immediately practical; they are intellectual or moral or aesthetic or, commonly, compounded of these qualities. In some groups these productions are very meager, as, for example, among certain hunters in tropical rain forests whose energies are absorbed in the effort to find food. And in some peasant or rural communities where food is plentiful one may find no art and little intellectual invention in the form of tale or myth. So it is fair to admit the great range of achievement with respect to these productions of the imagination and constructive power. But even in groups where these productions are meager it is common to find some occasional attention given to reflection upon elements of the local tradition, some discussion of the meaning of things, some enjoyment of a tale well told, or at least some silent relish of an experience, to represent the human disposition to use imagination, appreciation and discrimination in matters beyond material usefulness and practical necessity.

In effect I am saying that man is one kind of being while also this one kind is modified, developed or emphasized in particular

groups into many different kinds of being. I am also saying that the one kind of being he is while being also many kinds is a being composed of sentiments, desires and mental dispositions that animals do not have and that provide a basis, in some part of this nebulous whole, for any people to feel akin to any other. However exotic, however distressing or incomprehensible to us are aspects of another people's conduct, we touch them with likeness somewhere, somewhere in this "nuclear human nature." We may be so alienated from that other people by the obscurity or distastefulness of custom or character as not to find this place of likeness and sympathy. But probably no one who has lived in intimacy with an exotic people, in their homes, sharing their domestic experiences and coming to understand their public activities, has failed entirely to find it.

This is, then, an understanding of man's nature that sees it not as absolute or as somehow implanted, but as resulting from a specific inherent nature on the whole the same for all groups and developing contingent upon circumstances that differ widely and that nevertheless are everywhere broadly similar. It varies, but within limits that cannot be precisely defined. It has always within it a substantial part of a core of sentiments and desires, a view of life's significance and inherent rightness, a disposition to employ energies not demanded by the effort to subsist on works of imagination accumulating in the local tradition.

Being contingent upon circumstances that vary, it is a nature that can and does expand under some circumstances and contract under others. Where there is opportunity to use the mind and the feelings in some constructive play of fancy or intelligence, or to develop some heightened sensitivity, this nature of man expands, produces, over generations, notable works and becomes a nature that can be fairly, if vaguely, described as richer, deeper, more advanced. By this way of looking at the matter, some primitive groups may be seen as exhibiting instances of our humanity more meager, constrained and undeveloped than others show. The moral explorations of the Navaho or the intellectual achievements of Sudanese peoples described by Griaule and Dieterlen, are expansions of man's nature beyond what circumstances allowed to the Amazonian hunters described by Holmberg. The development of civilization from pre-civilized life is, for all man-

kind, an expansion of man's nature, an expansion marked by multitudes of inequalities of development as particular groups are compared with one another, and by regressions and contractions at particular times and places.

The contraction of our nature is most plainly seen in groups, not necessarily traditional, where special circumstances have removed parts of the characteristic nucleus of human character as it works itself out in tribes, peasantry, and civilizations. We see extreme contractions in samples of mankind reduced for long to hunger and weakness, or exposed to persisting exploitation and abuse, or subjected to torture. We can find contractions, probably, in traditional groups long exposed to hardship and prevented from experiencing the group life of undisturbed traditional peoples. Edward Banfield, reporting on his study of poor rural south Italians, described their morality as almost non-existent. He saw a people whose impulses had become limited to little more than prudential concern for survival of themselves and making some provision for survival of their children. He represents these people as without ideas of obligation, of moral concern, for neighbor or community. However much or little another look at these people would introduce qualifications of Banfield's view of them, the general appraisal no doubt rightly suggests what may happen to the nature of a traditional group when it is exposed for many generations to exploitation and hardship, and sees itself beaten down to poverty and loss of freedom while conqueror or rich exploiter prosper.

This conception of man's nature is derived, it should be emphasized, from that view of mankind which at least this one anthropologist gets by looking at the human examples characteristically studied by anthropologists. It has, therefore, its own bias, its own special contingency. It is based, on the whole, on what in these remarks have been more than once referred to as "the traditional groups." These are the primitive or tribal peoples and the peasant or other rural groups connected with the old-established civilizations. So the conception here put forward is of a human nature contingent upon the presence of an old local tradition, a degree of stability of association and self-definition of the local group. Looking at human history, past and to come, one recognizes a view more retrospective than prospective. Or it

may at least be said that it presents man as man has, on the whole and "so far," come about on earth. It presents what might be called the original formation of man's generic nature in historic man. It is not implied in this presentation that this nature is not changing under present-day changing conditions or that it will forever endure. If one turns to look at mankind in what is sometimes called "emerging world civilization" or in urban "mass society" one may perhaps get a different understanding of man. Looking at modern city dwellers, in all their diversity and in their separate individual states of mind—whatever are those states of mind—one would not, I think, speak as assuredly as I have here probably done of an ordered view of a significant universe. To many an individual not living in strongly traditional groups, the universe may look rather disorderly, or composed of bits of incongruous world-views. And the universality of a moral order might be found to require certain qualifications under present-day or developing conditions. The human sentiments and desires that connect in similarity one kind of human group with another probably remain today and tomorrow so long as there are intimate associations of men, women and children with one another. But as to what is happening or what is going to happen to man's nature in a long future, if one there be for mankind, I cannot, as anthropologist, say. It is the contingency of a common nature upon conditions that have prevailed in history, about which, in my special kind of experience, I can offer these large and—if you will—controvertible assertions.

A PHILOSOPHER'S INTERPRETATION OF ANTHROPOLOGY'S CONTRIBUTION TO THE UNDERSTANDING OF MAN

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My prescribed task here is to reflect for you and with you upon Robert Redfield's reflection upon his own and other anthropologists' contribution to the understanding of man. We must note at once that Redfield has placed his presentation within a certain conception of anthropology; a large part of his paper is devoted to delimiting the scope and method of anthropology. Well we have seen during the days of this institute that anthropology does not admit of being reduced to a neat, universally-accepted formula, of being cabined, cribbed and confined in a standard definition. It is, therefore, very proper for Redfield to set the limits of discussion within the reach of a certain kind of anthropology. Granted that other anthropologists may approach their subject in other ways, may repudiate, in whole or in part, Redfield's conception; it is upon his conception that our reflection here bears.

Now I, as a philosopher, am in somewhat the same position. There is wide difference of opinion as to the nature and function of philosophy; even within that ample and ancient tradition from which I draw my own inspiration, within Thomism itself, there are marked differences of opinion on these same points. Indeed, it is no secret that there are Thomists who maintain that Robert Henle is no Thomist at all. It will, however, have to be my philosophy and my Thomism (granting certainly that my Thomism may be a "free-wheeling" sort) which serve me as instruments in this reflection.

Well then, it is understood that this will be a confrontation between Robert Henle's philosophy and Robert Redfield's anthropology.¹

And so, what sort of anthropology is Robert Redfield talking about? Can we find any clues to a formal specification of what he describes as his anthropological approach? He tells us that

¹ The reader should consult, as a valuable supplement to Redfield's present paper, his article of 1957.

his conclusions will be drawn from what he takes to be "central" in anthropological approaches, namely: "the study of entire cultures or societies," of the "total, traditionally organized life-ways of many a human group." The approach is to the "totality"; the unit of consideration is a "whole way of life." Redfield himself clearly stresses the study of "whole" societies; but there is something else implied here, necessarily involved and clearly shown in the works of anthropologists, something just as important as the notion of totality. The anthropologists (of the type, at least, described here by Redfield) study this totality in its *concrete complexity*. Abstraction, indeed, is not necessarily opposed to "totality"; there is a sense in which the abstractions of the social philosopher express the totality of the social order; but the philosopher does not deal with the concrete unity and complexity of society as does the anthropologist. Anthropology is not simply an understanding of total social structure; nor is it a theoretical x-ray of society, such as one may find in sociological theory. Indeed, anthropology may develop such theories, or at least move toward them; it may also borrow its theories, models, techniques and so forth from other disciplines, but these theories and methods are always deliberately brought back to and related to the "whole." As Redfield says, they are used and understood by anthropologists "under the control of the entire life of the groups." One may stand back, analyze into separate patterns and functions, look at the economics, gather statistics, give psychological tests; yet to meet Redfield's description, he must replace all this within the concrete whole.

In this connection, we have been here discussing the difference between a clinical and a statistical interpretation of, say, tests. I should like to suggest that as the clinical use of techniques broadens out the interpretative framework of tests, which may be statistically interpreted more narrowly, so the anthropological use broadens the clinical into an "entire life" background.

Speaking philosophically but without pretending to sharp accuracy—I would say that the intellectual control of or with the entire group, in its wholeness *and* in its concrete real complexity, may be considered one of the formal specifics of anthropological understanding.

If this is so, Redfield's brief description of the sort of data

gathered and employed by the anthropologists is wholly consonant and consistent. Redfield speaks of looking "out" at people. The anthropologist goes into a community and "looks"; he watches behavior; he hears what is said; he observes children at play, families at meals, men at work, men and women at prayer and engaged in social and religious ritual. The data is objective; it is "other." When Redfield comes to draw conclusions at the end, they will not be brought forth out of self-contemplation but from the collected and collated data of human societies through the world. Yet there is a qualification on this objectivity.

For, true though it is that the conclusions do not come from self-knowledge, nonetheless they cannot be reached without introspective knowledge and without using one's self as an instrument for interpretation and understanding. The anthropologist uses his own humanity and his experience of his own humanity as instruments for the understanding of the concrete behavior which he observes. This I think is a precious and important observation. It would be well to pause and reflect upon it. Here is the anthropologist going into a community and observing what is going on. He watches a burial and sees a widow weeping and wailing beside the open grave. This overt behavior is, in itself, ambiguous. It may well be that she is happy to be rid of the old gentleman, but she can't precisely show this at the grave, before friends and relatives. She may, then, be simply observing the cultural rules of behavior rather than expressing "tragic emotions." Now, even the realization that the situation is ambiguous presupposes some knowledge of ourselves as human beings. If the anthropologist had no self-knowledge whatever, he would be unable to understand and interpret this situation. The anthropologists must use an imaginative projection, a sympathy, what is often called "empathy." This empathy is possible only where there is some community of internal experience. There is, to a very limited extent however, some sympathy between us and our animals. The dog wags his tail; we have some idea that he is glad to see us—but just what his experience of gladness is like we hardly can imagine. The little boy can have a certain "empathy" for his dog when it has been frightened. But what of the feeling of the setting hen; she just "likes" to set on the egg—but what sort of inner experience does she have? Her inner life is pretty much a

closed book to us. And so would the lives of other persons and peoples be, had we no community of inner experience, no common humanity such that, through our own humanity, we could make an imaginative projection and thereby interpret the behavior of others. We need common experience, disciplined imagination, and out-going feeling in order to "look" as Redfield's anthropologist "looks." This is the qualification Redfield places on the objectivity of the "data"—I do not however believe that this qualification necessarily destroys the objectivity or substitutes for it a vague and romantic subjectivism. I shall leave this point rest here for the moment; I shall return to it later.

Now I should like to say that in my view of the matter, the anthropological discipline described by Redfield is an empirical discipline, and I want to make quite clear what I understand by "empirical." In this whole area of epistemology, terminology has gotten so mixed up: words, originally perfectly sound and useful, have acquired so many historical connotations that it is extremely difficult to find words expressive enough to say what you want to say and neutral enough not to betray you or mislead your hearers.

"Empirical," is, philosophically, a perfectly fine word which simply relates to experience in a broad and indeterminate way. It has, however, come to have a more limited meaning and several different connotations; it becomes necessary, therefore, to find another word to convey the broader meaning. Experimental might have done very well,—but it, in its time, has been narrowed down to the notion of "experiment" as found in the physical sciences. I have been forced to employ another word to express the fundamental and universal relationship of knowledge of the real to experience. The word I usually use is "experiential." By calling a piece of knowledge "experiential" I simply mean that it is derived from experience, ultimately from a cognitive contact between the knower and some reality. The term thus excludes theories of *a priori* knowledge. The little child when he comes to school or, at least, when he starts out as a baby, isn't really packed tight with ideas, definitions, insights. It isn't our job as teachers, somehow, simply to open him up and pull out the ideas as though he were a well-packed little valise. "Experiential," as I use it, therefore, means that knowledge depends upon a first-hand contact between the knower and some object of knowledge.

Now, then, I wish to maintain that sociology (by any of its definitions), anthropology and philosophy (as I understand and profess it) are all experiential disciplines. But I wish to add, that anthropology, as described by Redfield, is empirical whereas philosophy is not. What then do I mean here by empirical? I am using the term to differentiate disciplines according to the evidential basis for their generalized judgments.

In dealing with any judgment there are at least two questions which we can ask. First:—What does it mean? “Meaning” is today a high-disputed and rather confused matter, but let us here be content to use a very simple everyday sort of “meaning” for “meaning.” If we say—“He is angry”—well, what does this mean? I surely have to understand what it means to be angry; ultimately, I have to reach some experience from which I derive an understanding of “anger.” (We may be very peaceful sort of people, yet we have enough emotion and enough of the beginnings of anger, to know what it is.) It is this sort of experiential source that enables us to understand what is meant by Redfield’s generalizations. When, for example, he talks of a dimension of morality in every society—not that some precisely defined moral act is everywhere to the same degree praised or blamed—but that there be some kind of “ought” recognized in every culture, we have to understand what is meant by this “oughtness.” This we do, at least ultimately and in its basic meaning, from our own experience; were we ourselves moral imbeciles, with no notion, no experience, no inference even of what “ought” is, Redfield’s statement would be gibberish to us. The aspect of meaning—as aspect of any judgment or proposition—is the first question we can ask about any judgment; this question, though presupposed, does not however determine whether or not the proposition is empirical.

For this, we turn to the second question—Is the proposition *right*? I may well know what it means to say “He is angry” or “The view of things of every people includes rightness and ‘oughtness’” and still ask—*But is it so?* If it is so, then upon what sort of evidence does your knowledge of *its being so* rest? Well, what is the evidence Redfield gives for the generalizations he proposes? It is simply that every society examined by the anthropologist displays this dimension; he speaks of “every

people", the "people of all such groups". The evidence is simply that every society, when examined, does, as a plain matter of fact, display a moral awareness. The generalization refers to such facts and logically is as good as the given range of facts. These propositions are, therefore, experiential and empirical. I am not saying this with the intention of either devaluating or inflating them. I am merely trying to identify the sort of knowledge we have here.

To resume and extend the specification of Redfield's anthropology as it is being revealed by our reflective analysis, we can now say that it is a discipline which studies human societies in their wholeness and in their concrete complexity, using objective data but employing as an instrument of interpretation and understanding the anthropologist's own humanity and knowledge of that humanity; a discipline, finally, which arrives at generalizations by an empirical collation of fact.

We can now turn to another qualifying remark. There is a paragraph in which Robert Redfield points to what he considers a limitation on the common practice of anthropologists. He explains that usually the anthropologist does not explore the sort of thing that Freudians and depth psychologists look to, that the darker inner impulses of human life are not plumbed. I am not prepared to agree without qualification that this makes the anthropologist's work more "superficial". Let this go, however, for I am more interested in Redfield's next remark. He says that some anthropologists have been concerned with these matters, "but, if so, their language and ideas have not been developed from their own comparative study of societies and cultures . . . they (the deeper depths) are reported in terms borrowed from the depth psychologist himself."

As I understand this paragraph, Redfield is setting up a distinction between what I shall, hereafter, call a "pure" anthropology and a sort of hybrid discipline. The kind of anthropology Redfield describes and practices does indeed develop its ideas from a study of cultures and societies.

Now there are a number of ways in which borrowing can take place. It is possible to borrow from other disciplines things which may be used as clues or techniques or sheer fact-finding instruments. Thus the anthropologist can use psychological tests

or sampling techniques; but if these are always kept in subservience to a consideration of the totality and the concreteness of a society, they need not dilute the discipline itself.²

There are no doubt problems and dangers in this sort of borrowing, but, in the large view, it leaves the integrity of the discipline untouched.

The situation is quite different when theories, concepts, principles are taken over from other disciplines and incorporated in the guiding hypotheses, theories or explanations of a discipline. The result here is not a mere juxtaposition or addition but a blending such that the discipline taken as a whole is formally different throughout. One cannot distinguish at any one point what is derived from the borrowed element and what has come up from the discipline itself. This I will call a hybrid sort of discipline.

Such a discipline presents a series of problems. The hybrid discipline becomes as viable as the borrowed theory; if the latter is erroneous and mistaken, unbalanced and partial or only incomplete and changing, the hybrid discipline runs the risk of having for *constitutive* principles errors and half-truths. It is difficult to keep reconstructing a discipline in the light of developments in other fields. Moreover, there is always the personal difficulty; an anthropologist will ordinarily not be trained in philosophy or psychiatry to the point of competent criticism; he is liable then to be the victim of the field from which he is borrowing. He may not have a satisfactory understanding of the borrowed theories or his knowledge of them may be outdated or drawn from too partial a set of sources.³ Such workers often are using an older, outmoded version of the discipline from which they are borrowing.

I am happy to find Redfield favoring the "pure" type of anthropology. If anthropology has any sound, independent sources of data and of generalization, it will build much more

² When the historian uses the disintegration of certain chemical elements to subserve his establishment of chronology, he does not thereby make his history a "physico-history."

³ Zirkle's remarks (1955) on "sociological biology" are here pertinent: "Biology is found in the elementary textbooks on sociology. . . . It is not the biology of the biologists, however . . . it can be described only as sociological biology . . . it is a law unto itself."

firmly and profitably by working up from its own *given*, even though it may never reach the neat, closed-in perfection of a systematic synthesis.⁴

We are now ready to look at Redfield's attempt to determine the nature of anthropology by relating it to "science" on the one hand and to "humanistic learning" on the other. Here is his basic statement: "We are, then, scientists, as is the biologist (1) in our assumption that there is a real world out there accessible to our observation, (2) in our reliance upon observation and inferences guided by reason and observation for our conclusions, (3) in our attempts to be objective and critical of our formulations, (4) in our responsibility both to the facts and inferences we alone have made and stand witness for, and (5) also to the correcting judgment, over time, of the scientific community." Redfield concludes that anthropology is, therefore, a "science"; yet, he at once qualifies his conclusion, ". . . a science *with close connections with humanistic learning*."

First, I must remark on the confusion surrounding the word "science." There is a traditional sense for this word, — *ἐπιστήμη*, *scientia*, science which was established for us in the *Posterior Analytics* of Aristotle and was there presented both as problem and as a theoretical description. The ideal description of science laid down by Aristotle was broader than the usual modern

⁴Leonard Bloomfield (1933) cites his own earlier presentation of the science of language as an excellent example: "In 1914 I based this phase of the exposition on the psychologic system of Wilhelm Wundt, which was then widely accepted. Since that time there has been much upheaval in psychology; we have learned, at any rate, what one of our masters suspected thirty years ago, namely, that we can pursue the study of language without reference to any one psychological doctrine, and that to do so safeguards our results and makes them more significant to workers in related fields. In the present book I have tried to avoid such dependence; only by way of elucidation I have told, at a few points, how the two main present-day trends of psychology differ in their interpretation. The mentalists would supplement the facts of language by a version in terms of mind,—a version which will differ in the various schools of mentalistic psychology. The mechanists demand that the facts be presented without any assumption of such auxiliary factors. I have tried to meet this demand not merely because I believe that mechanism is the necessary form of scientific discourse, but also because an exposition which stands on its own feet is more solid and more easily surveyed than one which is propped at various points by another and changeable doctrine."

There is a partial analogy here to the Aristotelian and Medieval theory of the *scientia media*, a science which borrows its formal principles from another science and, in and through them, organizes its own matter.

For a modern explanation, see Maritain (1946:83-89).

description.⁵ The thirteenth-century Scholastic thinkers saw a way to bring even theology — or, at least, a certain type of theology—under the Aristotelian definition. This widely diffused meaning (science₁) has been counter-balanced by the emergence of a new type of knowledge, originally called “philosophia” (as in Newton’s *Principia mathematica philosophiae naturalis*) but now known as science. The situation, however, is still more complex. “Science” and “scientific method” may be used in a very restricted sense to apply typically to the physical and biological sciences (science₂), extension of these terms to sociology, etc. being allowable only through assimilation to the former disciplines. Or again—a broad general notion of science (science₃) as “objective”, “based on fact” etc. is current where an *ideal* of knowledge rather than a formal description of a *kind* of knowledge is in the forefront of consideration. Now, these last two uses of the term “science” are frequently identified, either consciously and deliberately, by the adoption of an epistemological theory which allows no other respectable intellectual method except that of the physical sciences; or unconsciously, through the influence of our ambient culture, a culture in which “science” has achieved such overwhelming prestige.

Now Redfield is here attempting to “place” anthropology as a discipline by using “science” and “humanistic learning” as the two coordinates. I submit that in so doing he falls into the confusion of identifying science₂ and science₃, though his description of “science?” is rather of “science₃” than of “science₂”. Let us then return to his sentence.

The first characteristic he refers to is, I submit, not at all requisite for science₂ or science₃. Of course, I accept the reality—the “real” reality if you will—of a world “out there”, but a physicist, as such, need not do so in order to maintain and develop physics. A philosopher?—yes. A man, *as a man*?—yes!—but a chemist, a geologist—no. The point has been made dramatically. Consider two geologists—one who is also a Thomist and a philosophical realist, the other who is an idealist after the manner of Berkeley. They undertake, as geologists, to study the same mountain in Africa. They collect essentially the same sort of equipment, take the same boat, use the same sort of pick, study

⁵ See Maurer (1953:vii-xxxvi).

the same rocks, outcroppings, formations, and make the same sort of report to the Smithsonian Institute; in fact, simply confirm each other's findings. It doesn't make a particle of difference to them as geologists whether this mountain is, as the Thomist thinks, up there and stays there when he goes home or whether it is a complicated kind of picture-play which God puts on in our minds. Well then, science, I think, is self-contained enough and existentially abstract enough to proceed without even raising this sort of question.

With regard to the rest of Redfield's characteristics (2-5 in my numeration), I submit that these are descriptive of science₃ and are not restrictive. For these are simply characteristics of honest and capable intellectual work and do not, for example, differentiate essentially the work of the medieval scholastic thinkers from that of Galileo and Newton. There is a sense, as I shall point out later, in which "humanistic learning" as well must display these same qualities.

If then "science" is taken to be "science₃", it is too broad to describe a given discipline. If now "science" be taken as "science₂", I would argue that the use of two coordinates is inaccurate and inadequate. We are here brought back to the discussion of Dr. Lang's paper. Modern culture, I would argue, displays three, not two, basic intellectual attitudes in the elaboration of natural rational knowledge, the scientific (science₂), the philosophical, the humanistic. Although Redfield does hedge about his assertion that anthropology is a "science", I think he has succumbed at least a bit to the "scientism" which is a cultural determinant in our modern western tradition.

Let us for a moment consider the humanistic qualification advanced by Redfield. He says that anthropological reporting is not wholly unlike that of a novel or a biography. I agree that this is a characteristic of both anthropological reporting and interpretation, and, moreover, I would argue that, if anthropology is to be the sort of discipline we have found Redfield describing, this approach is formally essential to it. If the anthropological way of working is to lead to an understanding of human beings in concrete cultural contexts, the humanistic mode of understanding must be part of its approach. I suspect many people would feel that it derogates from the position of anthro-

pology as a discipline, at least as a *respectable* discipline, to say that it is much more like a novel than like a theorem in Euclid's Geometry or in gross mechanics. Don't we thereby deliver anthropology over to romanticism and subjectivism?

The criticism must be met, though I think it arises from a very narrow conception of intellectual "disciplines" (from, in short, the "scientistic" conception) and from a consequently very narrow conception of testability and validation. One thing surely that the newer phenomenological approaches and existentialism are showing us is that there is a *subjectivism* which is not *subjectivistic*. Certainly, in the humanistic methodology one cannot achieve the cold, calculated "objectivity" of mechanics. Though there is talk of the observer's getting into the observations even in physics, we can nonetheless get a pretty objective kind of situation when we are clocking billiard balls down an inclined plane or measuring the rise of mercury in Torricelli's experiment. Obviously love in an Indian village and prayer among the Yamana of Tierra del Fuego could not be reported or understood in so impersonal and "objective" a fashion. Here once again the observer must use his own humanity as an instrument, a formal methodological instrument, his own inner, "subjective" experience as a guide to interpretation. But even so, this is subject to the check of other observations, inferences, and the criticism of scholars: it is not "romantic"; it is subject to criticism and control and has its own sort of validity, as do dramas, novels and poems. A great novel portrays, indeed, a fictional set of characters and situations; but these must be authentically human. As the anthropologist displays and interprets a particular form of human living, so the novelist, if he is sound, portrays a possible form of human activity. The novels that are authentic literature, that endure through the changing tastes and probing criticism of succeeding generations possess their own validity as insightful interpretations of human life. We cannot write a novel about the billiard ball that refused to take part in the experiment. "I won't go down," said he, at the top of the incline. We can't write a novel about animals without making humans of them; in all the folk tales the animals are more human, one might say, than the people.

There is, then, a disciplined and responsible way to work

within the humanistic mode. I should like to suggest here that the anthropology which Robert Redfield is trying to describe is found to display a methodology that is not simply "scientific" with "leanings toward humanistic learning." I repeat, I think Redfield is here handicapped by using an inadequate, culturally determined epistemology. Anthropology may turn out to be a distinct disciplined type of humanistic knowledge with its own proper materials and methods, controls and theories.⁶ In any case, Redfield is describing a kind of anthropology which is "pure" in the sense that it is not structured and controlled by borrowed theory, which is not scientific in any narrow restrictive meaning of the term, and which is intrinsically humanistic in general character. Frankly, I am most sympathetic with this conception of anthropology.

At this point I must digress a bit and deal in a deceptively brief fashion with some very difficult topics drawn from epistemology.⁷ I have asserted above that at least three basic modes of knowing must be taken into account, the scientific, the philosophical and the humanistic. At this point I wish to draw a difference between the scientific and the philosophical modes by distinguishing their respective types of conceptualization. Let us take "concept" to mean the intellectual grasp and "mental" expression of some meaning or understanding. When I understand the word "angry" or "house" or "triangle", I am grasping the meaning of the word; yet this meaning is not something about the word itself or about my mental operations, it is about something else, something in reality or at least something different from the word and the act of understanding "anger" as it can be found in human beings, a "house" as a real dwelling place for man, a "triangle" as a certain precise object of mathematical thought.

All such conceptualizations are derived ultimately from ex-

⁶ There is an interesting similarity here to the epistemological status of Freudian psychology and psychoanalysis. Scientific-minded people have brutally criticized these approaches as lacking "scientific" form. It may well be that both in psychoanalytic theory and in anthropology we have emerging forms of a new type of intellectual discipline which, in its turn, will correct, limit and complete the predominantly scientific thinking of our recent past. For stimulating suggestions on these points see Stern (1954:149-77).

⁷ For the general theory of the disciplines here presupposed see Klubertanz (1955:89-104); Henle (1956:4-6); Maritain (1946:266-484).

perience! The child grasps things experienced, their manifest qualities and operations; he learns to ask for water, for milk, for candy, for play; and though he is interested in the things, he holds them in concepts. The store of concepts thus built up to make things manageable and ordinary life livable is not such as to constitute a "discipline". Somehow, concepts must be elaborated, refined, developed. Now we find, if we examine our conceptualizations reflectively, that our concepts move towards a pure intellectual state in two different ways. The concept may retain and express simply and only an intelligible grasp—however intricate or refined—of the *nature* of some real kind of thing or situation or operation and so forth. A simple, unrefined example would be the child's grasp of the kinds of "shapes" displayed by the blocks with which he plays.⁸ When the child uses the word "round", he is expressing only the quality of the block itself, the very kind of shape which it actually has. There is a direct *nature* relationship between the quality understood and the quality existent.

This sort of concept I call "ontological."

Now as we elaborate concepts to deal with things or situations where we are unable, either at first inspection or after long investigation and reflection, to grasp the *objectively given nature* we come to use substitutes for that nature, short-hand symbols for the complexities of the real, ingeniously devised schemata, models, and so forth to hold the real under control. Thus the concept of an "index of living cost" is an intelligible concept which does not express an intelligibility that as such can be found or made outside of my knowledge. Many factual elements have contributed to it; but it is formally constituted by a selected and devised manipulation. (Thus the concept of "ether" in nineteenth-century physics, of the "censor" in Freudian psychology, or the "field of force" in modern physics, etc.) Such concepts I call *constructural*.

⁸ These are not the "shapes" nor the concepts of shapes with which the geometer works. Euclidean geometry has become so diffused in our thinking that we find it hard to realize that there is any other way besides a geometrical way of thinking "shapes." I am talking in the text about a direct and primitive apprehension of shapes as qualitatively different qualifications of real objects. I use the example of the child because *he* has not yet either been (1) confused by geometry, or (2) turned into an unconsciously universal geometrizer.

Now an organized discipline requires not just a set of concepts but a system of concepts, and a given system must display a homogeneity of conceptual mode within its main organizing concepts. A discipline, therefore, must be constituted largely by one or the other type of concept. I am maintaining that philosophy proceeds by using ontological concepts while science² is characterized by using constructural concepts.

However, I will assert, too, that the humanistic mode also is very largely concerned with ontological conceptualization, but differs from philosophy in that the conceptualization of humanistic learning is viewed always in relation to the concrete, the individual, the complex given, the unanalyzed totality and includes accidental, partial, or simply "given" relationships; whereas philosophy moves away from the concrete and the individual, shucks off the accidental and retains only the intelligibly and necessarily interrelated.

Let me take an example which will also bring us back to our main theme. The traditional definition of man—rational animal—is a philosophical definition. It is experiential indeed because its basic intelligible elements as well as its basic validation have been drawn from experience. But the philosopher retains in his understanding only what displays itself as essential to man as an intelligible nature; as he develops his philosophy of man only those connections are retained as intrinsic to the discipline which are seen to be intrinsically or essentially involved with this nature. Thus the Thomist finds human freedom or self-determination intrinsically related—not by a mere logical involvement or implication but by an involvement in reality grasped intelligibly—to the nature of man as rational. There may be—indeed there are—many different concrete forms that freedom and self-determination may assume under different cultural conditions—but to these philosophy pays little heed; it goes to the essential *nature* relationship and accepts this relationship not as resting on an *exhaustive* canvass of fact but on the *intelligible natures discerned and discovered in these facts*. This further clarifies also what I meant when I called anthropology experiential and empirical but philosophy experiential and non-empirical. The generalizations about man offered by Redfield do depend for acceptance upon a more or less exhaustive canvass of facts.

This brief analysis of types of concepts gives a sharp distinction between science₂ on the one hand and both philosophy and humanistic learning on the other—while the analysis of typical context and typical content of philosophical concepts begins at least to draw a difference between philosophy and anthropology.

Now I wish to make one further distinction in our understanding through concepts. We can say that there are two different ways in which conceptual understanding can grow. The first is by simply adding factors or notes or intelligibilities to the concept. What is a purple people eater? It has one eye—yes—one horn—yes—and it eats purple people. Well, that's what it is—and my understanding of it is just the addition of these descriptive bits. It is in some such fashion that the zoologists describe and classify animals. Does it have a backbone or not? Does it have three sets of legs or not?

Definitions here explain and expand themselves by *addition*.

But there is another way of developing understanding. This is by deepening our insight into any given intelligibility. If we take something like human love or anger or desire, we can lay out a definition which even a college student can understand and to which nothing really needs to be added or can be added except depth of insight. The difference between the insight into love of the fifth grade kid, of the teen-ager, of the mature man and woman, of the Saint illustrates the possible levels of such understanding. This sort of deepening is the experience of philosophers, of artists, and of humanists.

Now it seems to me that the kind of anthropology which Redfield proposes precisely involves this sort of deepening understanding of man and of the "common human". As a philosopher I am confident that I understand the essential traits of what is "human". When Redfield tells me that everywhere man finds some significant order in the universe, I can quite understand why this is so and why it attaches so universally to human cultures—and this, though Redfield proposes that proposition as empirical while I would defend my position on quite other grounds. And so too when he concludes empirically to a common moral dimension, in fact also—so it seems to me—to a common metaphysical bent (or, as the judge's wife said in *You Shall*

Know Them, to a universal occurrence of a *juju*⁹), I find these but completions of philosophical knowledge. But when we turn to the full content and insight which the anthropologist brings to these conclusions from his experience, personal and vicarious, of widely differing cultures scattered across the globe and up and down the centuries, we will find, I think, a whole new dimension of understanding. In a sense, the philosophical definition of man lays down in principle the potentialities of human development; the philosopher cannot predict or deduce the historical forms and cultural actualizations of human potentialities and so forth; his understanding lacks content, for potentialities known only in general principles are not truly understood; it is the concrete, definite actualization that gives genuine insight into potentiality. I do not mean to say that the philosopher knows only a potential human nature; I do mean to say that he knows the *particularized* potentialities of man only in principle. He cannot, therefore, establish the development of historic man, the contractions and expansions (to use Redfield's phrase) of human nature through the varied circumstances of rain-forest and arctic waste, of nomadic and city life, of devolution and revolution and so forth. Redfield expresses this well:

"In effect I am saying that man is one kind of being while also this one kind is modified, developed or emphasized in particular groups into many different kinds of being. I am also saying that the one kind of being he is while being also many kinds is a being composed of sentiments, desires and mental dispositions that animals do not have and that provide a basis, in

⁹ (Sir Arthur's wife is speaking.) "I am not pulling your leg," she said. "Everybody has the *ju-jus* suitable to his age, I think. . . ." "You do need *jujus*, don't you, once you believe in something? . . . Even those bright people, I mean, who pretend they don't believe in anything, we see them seeking for something, don't we? They study . . . physics or . . . astronomy, or else write books: these are their *ju-jus* of a sort. . . ." "But if people really don't believe in anything," she said, ". . . if they have no *jujus* at all . . . then they have never asked themselves any questions, have they? . . ."

(And later in court the judge, Sir Arthur, is asking—)

"But they (the animals) do not have *jujus*?" asked the judge.

"No."

". . . They haven't asked the same questions."

"That is true," said Sir Peter. "The metaphysical mind is peculiar to man. The animal doesn't have it." Vecors (1953:163-64, 171).

some part of this nebulous whole, for any people to feel akin to any other."

If I may now try to sum up this point: The philosopher, indeed, grasps the essential nature of man (in the intelligibility of that "one kind of being he is") and deepens this, indeed, in principle and through insight into the essential ("per se") interrelationships of man's various traits—self-determination, rationality, morality etc. The anthropologist, indeed, comes by an empirical road, by a way of humanistic insight to a rather more nebulous and ill-defined realization of the common-human—but brings to this the *deepening* of his understanding of the "human" by seeing its "forms", the "human examples characteristically studied by anthropologists." The philosopher may be able to say, for example, that "art is the signature of man," but he has no way of deducing the forms and fashions which have *been* human art in fact; nor has he any way of incorporating the resulting insight formally into his philosophy. But to understand human art thus richly and fully and so also the men who have created it—the cave dwellers of Lascaux and Font de Gaume, the Greek vase painters, the craftsmen of the African figurines, the classic painters of China, and today's abstractionists—this is to understand man as artist: man indeed, and yet, a myriad of men, human even in their very differences; and this is the understanding of man as artist to which the anthropologist comes.

It seems to me then that the sort of anthropology described by Redfield comes to two different sorts of understanding. There is the rather obvious procedure of looking for what recurs, of crossing off the differences until one arrives at a set of empirical generalizations that rest on a check list of known cultures. In this way one comes empirically to some traits of the "common human". There is the deepening of understanding of the "human" that comes simply from the kind of experience which is characteristic of the anthropologist, a reflective penetration of the concrete, the particular, the different which finds humanity not as a mere common *residue* but as *present* in and understandable in all its varieties of contraction and expansion, of concrete actualization.

I would wish to stress this point. Perhaps anthropology's

greatest contribution to the understanding of man is not its derivation of impersonal generalizations about man but its grasp of cultures as totally and concretely and particularly *human*. Man is revealed in his every act, his every creation; even his sins display both his grandeur and his misery, for the sinfulness of mankind is revelation of both man's goodness and man's degradation. Sin is possible only to man. Animals cannot sin; man alone is capable of tragic moral mistakes. These mistakes themselves reveal, therefore, the moral dimension of man, the depths and potentialities of man.¹⁰

Now I wish to return one last time to the nature of anthropology. I think that anthropology can make this sort of sound and independent contribution to the understanding of man to the extent that it remains at least in its basic structure the kind of intellectual discipline Redfield describes. It must, therefore, resist the constant pressure of our culture towards gaining respectability by becoming more and more scientific (i.e. "science₂"); it must also resist the temptation to *borrow*, as a structural principle or an organizing theory, a theory of man whether from Freud or theology or sociology or philosophy. Let anthropology, in its basic structure remain "pure", "humanistic", and therefore independent. It will then be able to *use* the instruments and theories of other disciplines and to cooperate with and contribute to them.

Moreover, as an educator—and since this is also an educational institute—I would like to suggest that anthropology may well be the most promising of the modern disciplines for general education. I regret the passing of the classics as a central instrument of mature education. But perhaps some of what we got from a profound classical education, from the great classical culture and literature, can be given students in this sort of humanistic anthropology. This anthropology might, indeed, be center and focus of an education, within the framework of modern

¹⁰ What I have said in these last few paragraphs may well be taken also as a comment on Redfield's remark (1957:151). "These two contradictory aspects of human nature—that men in every tribe or nation are all very much alike and that the differences among peoples are great and perhaps limitless—come to men at different times, for different reasons, and with varying emphasis on the one or the other. The prevailing emphasis depends in part, I think, on one's philosophical position. The man of positivist inclinations is more likely to stress the differences; *the Thomist, among others, the resemblances, the common human*" [italics mine].

disciplines and modern life, which would at once achieve much of the results of classical education while pulling together and balancing the social sciences, the older humanities, philosophy, history and even science itself.

It is not, therefore, without significance that so many competent cultural anthropologists have come from classical backgrounds. Indeed, we come again to the basic point. The anthropologist, in his method, must use his own humanity as an instrument to understand the people and culture he studies; if *his* humanity is a poor and underdeveloped thing—a “contracted” sort of human nature—the anthropological method in his usage will be feeble and fallible indeed.

I regret, indeed, that Robert Redfield is not here with us. We, who have read and discussed his works here, have come to a deep respect for him and his work. For we do find in his works a full and rich humanity, a warm sympathy and understanding for man, a hunger—though humbly and without contempt—for goodness. These are the personal traits, I think, which have made Robert Redfield’s humanistic anthropology so powerful an instrument for the understanding of man and his interpretations so authentically human and so continuous with the deepest and best traditions of Western culture.

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THEORETICAL METHODS AND APPROACHES TO THE UNDERSTANDING OF MAN

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I.

Kroeber (1957: 203) recently observed that Ethnology "is a growing body of information and [is becoming] increasingly coherent. We cannot be too boastful about it, but need not be apologetic. Our total positive knowledge now is several times as great, [as it was when he started more than 50 years ago] and the areas of total absence of information are much shrunk. Ordering and understanding have kept pace. . . ." It is in this spirit that we shall examine not so much the content of anthropological knowledge, to which Kroeber had primary reference, but its methods and approaches.

Anyone who has ever looked into an anthropological text book must have been struck by the fact that the subject matter, the focus of anthropological inquiry, is always twofold. There are usually some chapters on man the animal, while others deal with man the carrier and producer of culture. As Kroeber (1948, 1957) and Redfield (1946) among others have pointed out, there is always a tendency in anthropology to have the subject matter so bisected. This has given rise to at least two major interests in anthropology, both reflected in its specializations, namely, physical anthropology on the one hand, and cultural anthropology on the other. Nevertheless there are strong and recurring movements in anthropology which bring these apparent divergent branches together. One of the most important is the fact that the anthropologist is forced by the nature of his subject matter — man — to see him not so much as made up of elements, but to see him as a unity, as a whole.

An appreciation of wholeness of the subject is often, one might say, "forced" upon the anthropologist. The ethnologist who goes abroad to study an exotic people is struck by the fact that people not only look differently, but also act differently from those with whom he has been familiar. He is struck by the fact

that at times a people's physique and their characteristic behaviors seem to coincide and are limited to a geographic area. But then again, as his experience with other groups grows, he notices that this body-behavior concordance does not always hold, and he concludes that culture and racial type do not necessarily coincide. Much work on this problem of the relationship of biological race and of culture has been done in the past (Boas 1945; Ashley Montagu 1954; Klineberg 1935). However, this problem has been solved. The interest in the relationship of biological man and cultural man has shifted. As the depth of knowledge of man through biological science has deepened, so also has the interest in the relationship of man and culture. As Howells (1955: 227-236) recently suggested, the emphasis in the study of biological man has another dimension: his variability. Much of this renewed interest is no doubt due to the fact that physical anthropologists are dealing with human adaptation. But often such "animal" adaptation is not merely biological, but is cultural as well. Furthermore, in the studies of personality assembled by Kluckhohn and Murray (1953), it becomes evident that there is a convergence of the biological and cultural.

The primary focus of this discussion is, however, not on man the animal, but on the "humanity of man" as Redfield (1953: 736) put it. The theory and methods that will be examined will be largely from cultural anthropology, and from within that area, especially those which belong to that field often labeled "social anthropology." Within cultural anthropology there are at least three major interests or foci for investigation:

1. The study of a society, of social relations, and of the values which support such units, i.e., a culture;
2. The study of personality, which is not culture but which expresses culture, and through the study of which we may better understand culture;
3. An interest in the constants among all human beings, whether they occur in our society or in some exotic tribe; a return to the attempt to discover the universals of human nature. It was thought a hundred years ago that they were known, as when the concept of "psychic unity" was current.

The methods used by anthropologists to study these three kinds of problems are very difficult to classify. If by method we mean "an orderly procedure or process . . . [a] regular way or manner

of doing anything, a set form of procedure adopted in investigation or instruction . . ." (Webster 1957: 1548) then it becomes apparent that there are at least two kinds of methodologies: one concerned with the "logical character of the problems set and the arrangements of propositions" or more precisely with "the conceptions which allow us to characterize and compare" cultures (Redfield, 1955: 1); and the other concerned with techniques for obtaining the data which are to be ordered.

II.

Method permits precision communication and the sharing of acquired knowledge, both substantive and theoretical. Redfield (1953: 733-735) has provided us with a classification of methods, which he calls models, each of which is a way of organizing data. A model is not necessarily a biological or engineering analogue such as an organism, or a feed-back mechanism, but it is rather a way of conceptualizing and presenting data, or of organizing conceptions on the basis of some underlying theory.

The five models are the following: (1) The natural science model, (2) the functional model, (3) the logical model, (4) the aesthetic model, and (5) the symbolic model. A discussion of these, starting with the last and working back towards the natural science model, may provide an opportunity to make further methodological distinctions among anthropological approaches later on.

The Symbolic Model: In this view a culture is described through its symbolic representations. The attempt is to characterize a culture as a whole in terms of its epics, myths, dances, religion and so on. According to Warner (1941: 21-25) for instance, all societies possess symbolic systems (ideologies and their sanctions) which differ according to the kind of social organization of the group. An intimate relationship between the symbolic system and social organization is postulated. The symbolic system that he refers to is the "social logics" of a community which provides the rationale of secular and sacred sanctions for the maintenance of the social organization. Thus, symbolic systems, on a low level of abstraction, may be observed in terms of the gestures and forms of address in social interaction. At a more general level of abstraction, the symbols are those of

a social relationship such as that of husband and wife. At a still more general level, the family is conceptualized not only as a symbol of human life, but as a religious symbol, e.g., the Holy Family. And this may be part of still wider conceptualizations which fit into a religious system and which have important determining effects upon other aspects of culture.

Another interesting example of this approach is given by Miller (1955: 271-289) who more specifically examines and compares concepts of authority in a European and in an American Indian society. He shows that the structure of authority is horizontally organized among Indians as is also their conception of the universe, while authority in the European society is hierarchically organized as are also religious conceptions. Thus the two systems of religion are a reflection of or are symbolic of the socio-political organization of the two cultures compared.

Perhaps this approach is most clearly seen in studies of Australian ritual, dances, and social organization. Some, like Sharp (1952: 69-90) for instance, would say that through an understanding of the religious symbolism, especially as it is expressed in the dances, mythology, initiation rites, and kinship organization, one can get a comprehensive understanding of the whole of a native Australian. Among these aborigines the past is told in myths and tales, and yet is a mirror image of the present state of affairs.

As Redfield pointed out, there is of course some difficulty with this approach. It must assume that the symbols used are meaningful to the people under consideration. It also assumes that people are able to create symbols. There seem to be many situations in which these symbolic representations are no longer meaningful, or in which people are not particularly imaginative because they are too preoccupied with simple survival, or in which the symbols have been introduced from outside and lack a functional relationship to the rest of culture.

The Aesthetic Model: Under this class of approaches come those which Redfield has called "constructed work[s] of art." These are the kinds of studies which emphasize themes, ethos, pattern, or style. No doubt this way of thinking about culture is related to the fact that a good number of anthropologists have had their original training in the humanities, e.g. the history of art or literature. Critics of the anthropological approaches, es-

pecially this one, often claim it to be not only unscientific, but also inaccurate and untestable. The degree to which it is inaccurate is more likely a function of the person making the study, and it seems that on the whole the danger is not so much on the side of accuracy, but perhaps more on the side of what is emphasized and how it can be tested. Nevertheless this approach has been most fruitful for the creation of hypotheses.

Benedict and Mead are perhaps most representative of this approach. In their works the emphasis is upon the roles that people play as part of a larger setting, a stage, as Redfield calls it. So for instance Benedict in her *Patterns of Culture* (1934: 78-79) tells us that "The basic contrast between the Pueblos and other cultures of North America is the contrast that is named and described by Nietzsche in his studies of Greek tragedy. He discusses two diametrically opposed ways of arriving at the values of existence. The Dionysian pursues them through the 'annihilation of the ordinary bounds and limits of existence'; he seeks to attain in his most valued moments escape from boundaries imposed upon him by his five senses, to break through to another order of experience. The desire of the Dionysian, in personal experience or in ritual, is to press through it toward a certain psychological state, to achieve excess. The closest analogy to the emotions he seeks is drunkenness, and he values the illuminations of frenzy. With Blake, he believes 'the path of excess leads to the palace of wisdom.' The Appolonian distrusts all this, and has often little idea of the nature of such experiences. He finds means to outlaw them from his conscious life. He 'knows but one law, measure in the Hellenic sense.' He keeps the middle of the road, stays within the known map, does not meddle with disruptive psychological states." Thus we see here clearly the influences that have shaped her perception of a cultural reality. And in this framework she analyses Pueblo, Plains and other cultures in terms of Appolonian and Dionysian categories.

Here then the central idea is that these cultures are integrated around or explained by what may be called a dominant drive-configuration, a "leitmotif," that runs through them. It is proposed that in such terms cultures may become understandable. And no doubt our understanding of cultures has been furthered considerably. We no longer look at culture as so many traits or elements, but are now aware that we are dealing with rela-

tionships of parts and that these parts are integrated. But the question arises, how can we be certain that we find these kinds of "leitmotifs" in each culture, and do we have the criteria here that will make it possible for two investigators to make valid comparisons without having studied both cultures themselves? It seems that, although not a very precise tool, its use may give considerable insight into a culture, depending very much on the investigator's particular background and training.

Opler (1945: 192-206; 1948: 107-122) has offered a "thematic analysis" and has gone a step further by suggesting that there are several integrating forces in most societies, rather than one. His formulation is an attempt to avoid oversimplification and at the same time to be more specific. A theme is a "postulate or position, declared or implied, and usually controlling behavior or stimulating activity which is tacitly approved or openly promoted in a society." A theme "is identified by and directly related to behavior by its expressions, the activities, prohibition of activities, or references which result from acceptance or affirmation of a theme in a society" (1948: 120). But again, criteria are difficult to apply in practice and we are given little help in knowing how to isolate themes one from another, and in deciding which one of the themes is of greater importance than another. But this approach too, helps in extending the search for still other dimensions of culture.

The importance of all of these approaches, however, lies in the fact that they have opened up a new area of investigation which goes by the name of "national character studies." This again was started by Benedict with her *The Chrysanthemum and the Sword* (1946) and later carried on by Mead and her associates at Columbia. Benedict's book sketches the principal motifs of Japan. This is also an important step forward in that she went beyond the local community level and tried to deal with a whole civilized nation. Dealing with large culture areas was not novel; others including Wissler and Kroeber have done this, but the method, which concerns us here, is different. The emphasis is on the relationship of culture to the development of personality, without specific reference to any psychological framework. The importance of this work lies in the impetus it has given to current national character studies as described by Mead and Métraux (1953). These later studies do not alto-

gether fit the aesthetic model, because, on the level of techniques, of collecting and using data, they are highly sophisticated. In spite of some of the critical comments directed at Benedict's oversimplification, most are agreed that "... the battery of imaginative insights and concepts with which she interprets her data . . ." (Bennett and Nogai 1953: 406) are one of Benedict's greatest assets. Furthermore, of particular importance are the attempts to portray the *whole* of a civilization, — always a tremendously difficult task.

The Logical Model: In this approach, according to Redfield, major premises of a culture may be detected, and much of the rest of culture deduced from them. Furthermore, the elements of an integrated culture are said to have logical consistency. He was thinking about Whorf (1941: 75-93), and perhaps Trager, both anthropological linguists. The former suggested long ago that certain linguistic categories are related to modes of thought, e.g. in the English language he noted that the warnings not to use open fire around gasoline barrels were ignored when these were identified as "empty." He linked this fact with the way in which in English "empty" confers the idea of absence of the inflammable liquid. So workers presumed that an empty barrel, devoid of the inflammable substance was not dangerous. Unfortunately they had to pay the consequences for their reliance on this linguistic category through explosions caused by the fumes remaining in the gas barrels when open light was used in their proximity.

Another kind of model which may perhaps be included here is the ideal type. This has been employed by Redfield (1947) in his folk society construct and more recently in *The Little Community* (1955). The aim here is to test the existence of logically derived relationships between empirical uniformities. The little community is an ideal type which is characterized by distinctiveness, smallness, homogeneity, and self-sufficiency. Using these characteristics as the parameters of the little community, it is possible to work out the implications of such an analysis whether one looks at it from the point of view of an ecological system, a social system, through the analysis of an ideal person and his development in such a community, through the study of the history of the community and its relations to other communities,

or in terms of the characteristic world view of that community. The importance of Redfield's "methodological" book is that it points out the consequences of looking at a human whole from one or the other angle, and shows that no one approach ever tells the whole story. In other words, there are going to be different conceptualizations of the human whole depending upon whether or not one uses an analogue based upon the machine, the human organism, the statistical model, or a system of logically related propositions expressed in a community's world view.

In a different frame of reference and less holistically oriented is Murdock's *Social Structure* (1949). From a study of 250 societies he derives certain empirical uniformities. After stating all his assumptions, based upon theory from psychology, sociology, and anthropology, he develops a basic postulate from which he deduces 30 theorems. These theorems are tested by Murdock as to their validity against the evidence from the Cross Cultural Files. By means of these theorems he constructs "laws" of social evolution. These, however, are different from the kind of evolutionary laws of the last century, being more modest, and based on much more empirical evidence. This important contribution, though different from Redfield's, has helped to stabilize terminology, identified types of kinship structure, and has suggested consequences for social structure. It does not, of course, deal with all of social structure as the title of the book might lead one to think, but it is conceptualized in such a manner, though this is not explicitly stated, as to take into account other aspects of social life, especially those of an economic, a psychological, and an environmental sort.

Perhaps the contributions by the Harvard Social Relations Department published under the title of *Toward a General Theory of Action* (1951) fits the logico-deductive model most completely. In the introduction the aims or purposes of the book are stated: "Theory in the social sciences should have three major functions. First it should aid in the codification of our existing concrete knowledge. It can do so by providing generalized hypotheses for the systematic reformulation of existing facts and insights, by extending the range of implication of particular hypotheses, and by unifying discrete observations under general concepts. Second, general theory . . . should be a guide to research. . . . It enables us to locate and define more precisely the boundaries of our knowl-

edge and our ignorance. Third . . . [it] will facilitate the control of the biases of observation and interpretation which are at present fostered by the departmentalization of education and research" (p. 3).

The theory of action as elucidated by Parsons & Shils is summarized by Sheldon (1951: 42-3). The basic premise of this theory is that the actor strives to achieve goals. In seeking these goals he is oriented to objects, and the orientation is assumed to be in three modes: the cognitive, the cathectic, and the evaluative. These modes are the basic principles which relate actors to objects and so become the basic principles by which activity is conceptualized as action. Objects of orientation are assumed to be relevant in a situation because they allow alternative ways and means to achieve certain goals based on certain needs and also because they provide the limiting conditions in this process. This means that orientation of action toward objects entails selection. Actors, objects and modes of orientation, i.e. the principles relating actors to objects, are the basic conceptual materials of personality, culture, and the social system. Thus it should be possible to develop logically from these three categories and in terms of these basic principles a unified theory of action. The result is a very complicated classification and cross-classification of modes, objects, and alternative possibilities of selection among these which crosscut and intermingle with the concepts of personality, culture and social system. This, theoretically at least, allows for a tremendous number of kinds of action. The resulting all-embracing theory has, no doubt, very high potentialities, provided that it will help in giving factual meaning to these categories in terms of certain operations, and provided further that it will be possible to derive from these categories relationships subject to empirical test. So far, relatively few people have operated within this framework. But a number of researchers have at least used parts of the theory in empirical research. What the eventual outcome of the application of this theoretical system will be, remains to be seen.

We may briefly deal with still another attempt to build a logical model. Nadel (1957:6), a British anthropologist whose last work has recently been published posthumously, tells us that the purpose of his investigation into social structure is to "examine afresh the meaning which can usefully and logically be assigned

to [it].” In fact Nadel uses a system of notation borrowed from mathematics and symbolic logic so as to be able more readily to show relationships, especially when they are complicated and cannot easily be described in any shorthand fashion. This, he says, is justified because the phenomenon studied is formal and hence lends itself to mathematics and logic. Nevertheless, in contrast to Leach and Levi-Strauss, his theory of social structure is said to have an empirical reality (pp. 7, 149) and is not merely a construction of the mind. The social structure then is described in terms of roles. The role concept he develops logically from his own theory of social interaction. Roles are the stuff of which the social structure is made, we are told. It is too early to see how useful this conceptualization is, but it seems to be related to an ever-growing concern that the mass of social data be handled more systematically so as to provide a better check on the validity of our propositions and their interconnectedness.

The Functional Model: This way of looking at anthropological data is especially associated with the names of Malinowski, Radcliffe-Brown, and their students. Although there is often great difference of opinion about the term “function” — its uses and its applicability — it seems that some such conceptions of “function” are part of the conceptual inventory of anthropology. There are at least two principal meanings of the term in anthropological theory: (1) the term is used in a mathematical sense, that is to say, an interdependence of two variable qualities is assumed and in this sense it is of course related to the notion of co-variation; (2) the term has been linked closely with the problem of assignment of meaning in a broad sense. In this use, variation is not negated but it is suggested that varying elements have a greater or lesser significance. Function so conceived means that any cultural element is oriented towards given ends.

It seems fair to say that the second meaning is the kind that is more often discussed than the first. It raises also many more problems. So, for instance, to Malinowski, function is intimately related to the fulfillment of needs, either individual or social (1931: 629; 1936: 44). Implied in this notion is, of course, the existence of an equilibrium and the analogue from which these ideas spring is that of a living organism such as the human body.

Radcliffe-Brown, another "functionalist," regards functioning as the result of activity, as a process (1935 & 1952). But he adds an important dimension, the concept of structure, which always remained implicit in Malinowski. Function in this view preserves or maintains the structure. Viewed this way, an inefficient society is one where there is functional inconsistency. Functional consistency and inconsistency are important theoretical contributions because they suggest an awareness of the fact that not all elements in a culture are necessarily of equal importance to the functioning whole. Radcliffe-Brown does not treat function in terms of needs as does Malinowski, but in terms of the contribution a partial activity makes to the total activity of which it is a part, which in turn makes a contribution to the existence and continuity of the system.

But there are difficulties with this notion. Nadel (1951: 371) points out that it is difficult to objectively assign utility to a functional element. Merton (1949: 21-81) has also challenged the concept of functional unity. He manifestly denies that a modern society in particular has such functional unity. He then develops his own paradigm of functional analysis which has become important not only in sociology, but also in anthropology. Thus functional analysis involves the establishment of some functional requirements for the operation or the survival of a social system. It involves the relation of these functional requirements to a particular kind of structure; this in turn requires the description of the structure and the processes through which these requirements are met. Also it is necessary to look for compensating mechanisms if some of the typical functions are inadequate or interfered with. All of this led Merton to a more precise definition of function and to the idea of "latent function."

An important influence in social anthropology has come through the work of Levi-Strauss (1954: 105-106). He has been concerned primarily with structural analysis in which he has benefited by his association with Jakobson, a linguist. But he too talks about function, though he uses it more in the mathematical rather than in the empirical sense as did Malinowski. Important in his thinking is the fact that anthropology more than any other discipline has recognized that all aspects of social life constitute a significant whole, and that it is impossible to understand any one aspect without considering it in the con-

text of the others. Social anthropology tends to give a logical priority to the whole over the parts. Any cultural technique then, in this way of thinking, has not only utility but it also serves a non-utilitarian function. For a certain technique to be understood, for example, its social significance as well as the mechanics involved, even its history, etc., must be studied. And, in his view, linked with the variety of functions are notions of structure. The importance here, then, is that he points out the relational character of function and its intimate relation to social structure.

In America, too, functional analysis is being used, although more often implicitly rather than explicitly. More often than not, the usage is in terms of association of elements as in Spicer (1954) and Tax (1953). In the latter's treatment of the function of wealth in a Guatemalan peasant community he examines how wealth varies with different families and styles of life. Spicer speaks of functional linkages between aspects of culture among the Yaqui. Emphasis here is on configuration. Watson (1952), too, as part of the configurational analysis, examines the functions of culture in a synchronic sense, and separates it from historical, reconstructive and logical approaches to the study of culture change.

The purpose of such functional analysis is to establish causal relationships. A study in the framework of acculturation by Spindler and Goldschmidt (1952: 68-83) examines various types of social adjustments which are said to be a function of the degree of acculturation. Among the more topical treatments is an interesting study by Rapoport (1954) on changing Navaho religious values. He not only describes Navaho society, the missions and the responses of the Navaho to the proselytizing and to the changing conditions, but also the personal need-dispositions of the Navaho. He shows that conversion to Christianity is not an automatic process but reflects, and is a response to, socio-cultural disorganization and emotional needs of individuals.

Difficulties with functional analysis arise from the fact that often these studies do not make clear whether function refers to the individual, or to the society. As Firth (1955: 258) points out, it may be a legitimate assumption to hold that social and individual emotions may be equated in certain instances, but such equivalence is far from universal.

Although it has been said that the functional model is based predominantly on the biological or mechanical analogue, it seems from our very superficial examination of the studies of anthropologists working within this conceptual scheme that many workers do not employ it in this manner. Function is conceived of in terms of certain logical requirements which spring from empirical generalities or propositions. On the one hand those who work in the direction of the "logical model" and who deny any direct connection between reality and theory, employ the concept in the mathematical sense (Leach 1952; & Levi-Strauss 1954). On the other hand Nadel (1957: 158) says in quoting Eggan, that "social structures have certain jobs to do . . . hence some structures will be more efficient for certain purposes and less so for others." If function is substituted for "jobs to do" it becomes clear that the term is teleological in nature.

Functional analysis may be a long way from being the precise tool that the scientific pursuits demand, but as Firth's (1955) useful review suggests, we have learned more about society and culture. It may also be said that perhaps the functional model is of greater value in pointing out the steps that have to be gone through in the analysis, rather than the final analytical product. Through such procedures greater insights may be gained in understanding cultures.

The Natural Science Model: Anthropologists often think of themselves as natural scientists, even if their first training has been in the Humanities. To what extent this is itself a product of our culture where science holds such a sway is hard to assess. But it seems that most anthropologists at one time or another state that they are scientists and that their method is that of science, meaning natural science.

Redfield, in characterizing this approach, says that the natural science model conceives of a number of necessarily related steps: activities begin with a problem seen and conclude with a theory tested. There is little doubt that we see many problems. There is also little doubt that we would like to test many of our theories. It should be noted that I say theories rather than theory, because, in this respect, anthropology is most prolific. Furthermore, many of the theories which have been put forward are in fact not testable, at least not now.

We might perhaps begin with a statement of the aims and methods of natural science. Although prediction and control are often cited as sole criteria for this approach, certainly explanation and understanding are important as well. An explanation eventually may become a theory. If it covers a large number of phenomena, and if this explanation is general, it is said to be a good theory. It may be said that theory or general explanation is the ultimate objective of science. Furfey (1953: 56-86), for instance, points out that the qualities of scientific knowledge are three: certitude, causality, and generality. The steps which are essential then in the development of a scientific method are careful observation, ordering these observations into meaningful wholes or constructs, and relating them one to another into possible explanations or preliminary propositions which may be called hypotheses.

On a more technical level science requires that observations must be void of ambiguity and that the constructs developed must be stripped of any surplus meaning. With regard to the first requirement, it seems that certain controls must be introduced in the observation of facts. This may be done variously, but involves in particular the control of bias, and the necessity to achieve representativeness, and uniformity of observation. In regard to the second point, a natural science approach demands that concepts, which at first are no doubt general, insightful, and useful must be more clearly delineated. Concepts must have only one meaning and no ambiguous connotations. Ideally the natural scientist hopes to achieve this through certain operations, so that his concept may be said to be no more or less than a set of instructions. If these steps have been carefully noted, he then can combine the various empirically derived propositions in such a way so as to develop certain tentative explanations or hypotheses. These hypotheses presumably permit prediction. To the extent to which a hypothesis actually predicts, i.e. is verified, to this extent an hypothesis becomes a new scientific fact, and goes into the storehouse of other facts which will become part of a general theory. Thus testability of the hypothesis and predictability are often thought to be essential elements of natural science methodology. This implies, it seems, that it is necessary to have some notions of causality when examining relationships,

even if the investigator claims that he is only interested in correlation.

Now in anthropology as also in most of social science, this is the way that many would like to proceed. Certainly the search for causes in cultural and social phenomena has been underlying much of anthropological research. Nevertheless, much of anthropological science remains on the level of hypothetical knowledge, because even the most logically sound propositions do not lend themselves readily to empirical testing or to causal explanations due to the complexity of human life.

A few illustrations might help in pointing up some of the problems. For instance Kardiner, Linton, and their associates, developed a theory of basic personality in which it is assumed that certain social institutions have an effect upon personality and that personality affects other institutions. The propositions of this theory may be summarized as follows: Certain culturally established techniques of child treatment shape basic attitudes towards parents. These attitudes persist through the lifetime of an individual. Attitudes and behavior formed by the culturally standardized patterns of child treatment and which persist into adulthood form a "group of nuclear constellations" known as the basic personality structure which is characteristic of that society. The patterns of child treatment are called primary institutions. On the other hand, through the psychological mechanism of projection, primary institutions are reflected in the development of other institutions such as religion, government, law, etc. These are the secondary institutions. Thus we have a chain-like arrangement of causes. (Hoebel 1958: 582-3).

There are some difficulties with this theory, and criticism has been widespread. But we are primarily here interested in a method. What Kardiner has done is in fact methodologically correct. He has, or rather his associates have, made more or less precise observations of the patterns of child rearing and institutional life in a number of non-western societies. These have been grouped together under his two concepts of primary and secondary institutions, and he has constructed a hypothesis to explain the relationship between these institutions and personality. In fact, this hypothesis was worked into an explicit study and tested in Alorese culture.

Another example may be cited from Murdock (1949: 201-222) whom we have mentioned already in a different context. Here a causal relationship is established to the effect that certain residence patterns are determined by ecological factors. Due to these residence patterns, kin are associated in certain ways and thus particular kinds of social organization are produced. In a given case, the particular kind of social organization is reflected in a particular kind of kinship terminology. Based on earlier researches of Lowie, Titiev, and Kroeber, Murdock's theory, oversimplified here, constitutes a significant methodological contribution. But again, Murdock himself warns that this is all provisional, and must be subjected to more empirical testing. Why? In part because there has been ambiguity in the recorded observations both by amateurs and by anthropologists, and perhaps also because some of the concepts in kinship analysis are still imprecise, leaving the possibility of "surplus meaning." These observations have suggested a number of hypotheses which are testable. No doubt we shall continue to hear about them. Many, particularly younger, anthropologists have worked within the framework proposed by Murdock and have found it wanting here and there, but have also found it a useful heuristic device.

Still another causal model is that of Julian Steward (1955: 178-209). The very title of the paper, published earlier in the *American Anthropologist*, is suggestive of his purpose: "Development of Complex Societies: Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations." In this very provocative paper he examines the development of civilizations under arid or semi-arid conditions in Mesopotamia, Egypt, N. China, N. Peru, and Meso-America, and finds certain uniformities in this development. For instance, all seem to pass through certain eras of hunting and gathering, incipient agriculture, formative period, regional florescence, initial empire, dark ages, and cyclical conquests. He says (p. 199), "The successive eras in each of the five principle centers of early civilizations appear to have had similar diagnostic features which, arranged chronologically, might be considered as superficial formulations of regularities. Such a formulation, however, would fail to prove a satisfactory and generally valid functional explanation of cause-and-effect relationships between phenomena. To provide deeper explanations, it is necessary to make cause-and-

effect as explicit as possible and to test the explanations offered for the sequence in each center by the data of other centers." He then, in his summary, gives a "rough, cursory and tentative" explanation for this development. The scientific nature of his approach is suggested by the limitations of the explanations he offers, such as specified conditions of aridity, types of societies described, and the consequences of certain environmental factors which interplay with socio-political factors. All these factors are made as specific as possible within the framework of such a trial formulation. But, although highly stimulating, and perhaps testable, the concepts employed are often loose because of the kind of data one is forced to work with, data which do not permit greater specification. The facts used are limited and are themselves often of a hypothetical nature.

In the discussion of these models it becomes plainly evident that as a natural science, social anthropology has only begun. It is very careful about observation. The kind of controls that have been introduced into observation are in part due to the anthropologist's preoccupation with non-western societies where he can see things in a new and fresh way. Thus he is more apt to be aware of his own bias and to come without too many preconceptions as to what a society ought to be. He knows that there are a great many kinds of societies. In regard to description, he is careful, but not quite so precise as the natural scientist, his data are more complex, and as a result he often has to be selective and sometimes fairly abstract.

But as Kroeber (1957: 203) points out in his recent essay, "possession of an organized corpus of knowledge of phenomena is the precondition of any soundly growing science." Thus, the ordering that has taken place within the framework of the natural science model has constantly furthered the acquisition of data. At the turn of the century and after, when the ordering of data in American anthropology was predominantly in terms of culture areas, and the necessity of gathering data on still unknown tribes was urgent, anthropological field work received a tremendous impetus. Because anthropologists have a number of different preoccupations, or put another way, there are many more theoretical frames to work with, they have come to look at these data with a fresh look and find them at times wanting. More

and different data, depending on the theoretical orientation of the investigator, are still needed.

What then is the status of theory in anthropology? It seems that on the one hand there is a proliferation of theory in anthropology and on the other a paucity of actually tested hypotheses. This is an accurate statement if by theory one means sets of connected propositions. This assertion is supported by Keesing's survey of work on culture change (1953) where he points out time and again that there is no lack of theory, but that testing is woefully inadequate. Although anthropological theory and the related hypotheses are always anchored in some observed facts, they are rarely brought together. Neither are the elements of such theories often made very explicit, nor are the assumptions clearly stated. Perhaps in the field of social organization there has been greater preciseness than in any other. This may be related, in part at least, to the linguistic categories used in this work which lend themselves to a fairly rigorous analysis. A similar structural technique is now also employed, though modified, in the analysis of myth. Also greater precision is gained in the field of value studies. Kluckhohn (1951: 388-432) has set forth a conceptual scheme which will help in the classification of values through the spelling-out of criteria. Even in a preliminary form, it seems that the application of these criteria has already proven useful as, for instance, in the book by Vogt (1955).

III.

The five models that have been discussed above are obviously only one way of categorizing anthropological approaches in the study of man. There are a number of approaches which cross-cut these five modes of studying man in a more or less explicit manner. Some of these other approaches or methods have already been illustrated by the examples given, but they may be made more explicit now. The first is the comparative approach. It is certain that all of anthropology use this approach. Even when only one tribe is described in great detail, and no reference is made to any other material, the description is done with the intention of adding to the store of data which will eventually be used in some kind of comparison. Comparisons are made both synchronically and diachronically, and often the same

author may use both approaches as does Eggan (1950) in his *Social Organization of the Western Pueblos*.

The status of comparative studies has recently been reviewed by Lewis (1955: 259 & 292) in which the plea made by Steward and Redfield for clarification of cultural types is repeated: "for a marked improvement in comparative anthropology it might be wise to encourage our students to specialize either in tribal, peasant, or rural societies." The little community of Redfield seems to be a very promising tool for such a comparative analysis of small societies. His folk society construct has already been used by a number of students to some advantage, as also the extension of these ideas in his more recent *Peasant Society and Culture* (1956). Steward too suggests that the historically flat culture area approach must yield to a more dynamic typological approach. To him a culture type is characterized by selected features rather than by its total element content, since no two cultures will have the same kind of content. This calls then for some degree of abstraction so that fruitful comparisons can be made. As Redfield put it (1957:6): "The mind needs abstractions, those mental economies that stretch the truth and so make truth plain." This then is a recognition that each culture on the one hand is a unique phenomenon, but on the other that there are certain uniformities that impress themselves upon us as we go from one culture to the next, not so much in the detailed, concrete empirical fact, but in the way we organize these into "mental economies." Steward (1955:23) also observes that in the constructing of cultural types the selection of cultural features must be made in terms of the problem at hand and in terms of a specific theoretical framework. He addresses himself to the problem of cultural evolution and uses essentially an ecological framework or theory. The selection of these features is not random for each is presumed to be of diagnostic value and must have "functional interrelationship" in the culture itself and, one might add, to the theory as well.

But comparison has not only gone on in terms of cultural types, or wholes — whatever indicators may be used to characterize these — but comparisons have also been made in terms of certain institutions. Most outstanding in this respect is the recent book by Hoebel, *The Law of Primitive Man* (1954) in which he compares the legal aspect of culture among a number of se-

lected tribes. The categories for comparison have been derived from considerable experience in this field, both primitive and modern. But unlike many such topical treatments, Hoebel never loses sight of the fact that law occurs in a cultural context. Murdock's *Social Structure* is a specialized comparative study of the social aspect of culture. There is not space to give examples of the comparisons made, nor of the kinds of comparisons. But it should be noted that the comparative method is as essential to anthropology as experiment is to biology or physics. Comparison provides the kind of control which experiment does to other sciences. In anthropology, however, comparisons seems to have been of the following kinds: according to form; or according to function, quantitative or qualitative, morphological or statistical. Often, these approaches have been combined (Ackerknecht, 1954: 124).

In talking about comparison, either between cultures, between areas, within cultures, or of aspects of cultures, we note that there is another division which has been discussed widely. This division is between a diachronic as against the synchronic approach. In America, much more than in Britain, there has always been a great deal of respect for the diachronic approach. In Britain, in contrast, there has been a growing awareness that the application of the two methods is not diametrically opposed but is complementary. The synthesis of the diachronic vs. synchronic approaches has best been demonstrated, it seems, by Redfield in his own field-work and is discussed systematically in his most recent writing.

Much of the opposition seems to derive from the fact that those who lean towards the natural science approach tend to feel that a historical approach is non-scientific. The scientific view holds that phenomena are transmuted into abstract categories which are not concerned with the particular, as presumably history is, but with the general, and with processes. So conceived, this permits theories to be formed in accordance with the model that has already been discussed. The historical approach is identified with "descriptive integration," the reconstructive effort to preserve reality within its contexts of unique positions in time and space. And as Kroeber (1952: 160-161) points out, the historical approach also retains the qualitative reality of the culture described.

But such descriptive integration is not as devoid of testability as is often presumed by natural scientists. A test of the validity of the historical approach is provided by the degree to which the phenomena fit into the totality of the findings conceptually conceived. Descriptive integration is not, then, a mere chronological account of cultural phenomena but it is the ordering of these phenomena according to a particular conceptual scheme. The chronological aspects are important here, but the kinds of history that anthropologists write will be different from those of other historians precisely because of the ordering of data, and the manner in which they have been obtained. In much of anthropology the ideal of the natural science model has not been followed through to its logical conclusions. Instead, the method employed more frequently is that of descriptive integration. The reason for this lies at least in part in the subject matter itself, which is man, not matter, and which is much more complex than matter. Perhaps it is worth the trouble to examine the notion of "descriptive integration" more carefully.

Descriptive integration shares some elements with science. Both try to conceptualize, but the differences in the conceptualizations achieved by them is that science destroys individuality, while descriptive integration maintains a "phenomenological quality" (Kroeber, 1952:71). The essence of this approach is the building up of selected and reconstructed phenomena into significant conceptual relations or groupings. Thus the sequence of events is not as important as has often been suggested. In fact, much ethnographic material is descriptive and static. This is unavoidable in characterizing a period in the historic continuum. In this approach, process, which is vital to the scientific approach, is secondary. In descriptive integration one obtains a view of a culture which corresponds more directly to the reality of the phenomenon. In the scientific approach, the concrete, substantive reality is lost sight of in favor of some more general abstraction. This latter approach may be compared with geometry when cubes, circles, or triangles are described. Each one may be accurately described in terms of lines, angles, points and radii, and yet it does not describe any one concrete geometric figure until we specify it in terms of certain values for each.

This discussion suggests then, that there is a constant tension, often even an ambiguity, not only in relation to subject matter —

the humanity of man versus man the animal — but especially in method and theory. Our theory has as yet little in common with that of the physical scientist, in spite of the efforts in that direction. Certainly, there is no general theory as there is also no unified discipline, if it may be called a discipline at all. It is a field in which people are united because of similar interests, with a diversity of backgrounds and preoccupations. It is a field that is exploratory and probing. Characteristically, anthropologists attempt to maintain the unitary nature of man. Whether they study him as an individual or in the group, reference is always made to the whole, of which he is a part, and this whole will include, at least in the more sophisticated approaches, his history. It can be claimed that even social anthropology, which often has been described as purely synchronic in its approach, is both scientific and historical. Such social anthropologists as Evans-Pritchard (1950:120) have even gone so far as to insist that a science of man is not possible, and that only a history of man is the proper aim of anthropology. Nevertheless, natural science and anthropology shares these aims: explanation *and* understanding, based on observation, and through careful description to communicate its findings to others in the field and to those in related disciplines.

On the other hand, anthropology continues to derive much of its insights for description of human wholes from the humanities. But it goes much further than that. From the humanistic approach to man the anthropologist learns to appreciate the fact that man is not wholly describable in terms of variables, statistics, or other abstract entities, but that there are other dimensions which are not easily got at by non-humanist approach. Some have ventured to diagram this contrast between the natural science approach and the approach in the humanities.

	Hypothesis (intuitive)	Testability	Deduced (rigorous)	
"Humanities" approach	constructs (with "surplus meaning")	operations	clear cut and sharp constructs	"Science" approach
	observations (ambiguous)	control	comparative or experimental	

As one introduces more control, operational validity and testability, to that extent one becomes more scientific than humanistic. The distinction, however, is far from absolute and the scientific ingredients are often represented with different intensity and on different levels. Sometimes something is lost as one moves towards science; sometimes something is gained.

Social anthropology and anthropology in general are neither exclusively scientific nor humanistic. This seems to be a desirable situation. The polarity of approach may have been painful to many workers who have felt a need to be identified with the one or the other discipline. Nevertheless, these centripetal forces have also been effectively integrative based upon a twofold commitment of those in anthropology:

1. A commitment towards viewing man as unique and yet as one of many creatures to be studied as objectively as possible by the methods of natural science.

2. A commitment to view the unique humanity of man as consisting of persons who are moral, self-conscious, creative beings directed by ideals.

The consequence of this has been fruitful. Other specialists often tend, especially in the social sciences, to look at man as being represented by so many traits or variables. The anthropological approach has introduced in these specialties a warning calling attention to the great complexity of man in society and culture. Perhaps this complexity is responsible for the failure to develop a unified theory of culture. Possibly also, our knowledge as yet is too limited; and if there is no general theory this may suggest that anthropology is slow and careful in its formulation. It is not suggested that there should be no theorizing, but Merton's (1949) counsel, to concentrate on "theories of the middle range" is well taken. "Freezing" of theory too early might be harmful. On the other hand, data do require ordering if any sense is to be made out of them. It is for this reason that anthropologists must become more self-conscious of the role of theory in their work and of how it can help in achieving greater understanding of man.

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APPLIED AND ACTION ANTHROPOLOGY IN THE UNDERSTANDING OF MAN

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To locate the "applied and action" components of social anthropology as against its "theoretical or pure" activities is conceptually simple enough. When we think and talk about the investigation of the social and cultural life of human beings we tend to make a distinction between applied and theoretical research on three grounds: (1) the kind of problem, (2) the kind of conclusion, and (3) the kind of commitment. If the problem is a problem of somebody's everyday life, if the kind of conclusion is a rule of practice, and if the kind of commitment includes values in addition to that of making true sentences about the world, then we usually call the research applied, while if the opposite set of features prevails, we speak of pure or theoretical research.

In contemporary social anthropology there is a wide division of opinion as to how separate the applied and theoretical components are or ought to be in a particular research activity. At one end of the continuum is Evans-Pritchard (1952) who maintains that nothing "fundamental and practical" can be pursued in the same research enterprise, while Sol Tax (1952), at the other end, conceives a research activity in which helping people and contributing knowledge are wedded into a single enterprise.

Within the field of applied or action anthropology itself there is different sort of continuum reflecting the notions current about the relation of knowledge to practice. The range is from the scientific to the humanistic views of the role of fact and principle in conduct and policy applicable to organized social life. At the scientific end are those whose model of science and application is akin to the glamour science of our day, physics. For example, Chapple (1953) speaks of prediction and control based upon the utilization of general knowledge in a particular instance. In industrial and business applied anthropology, this image, application of principle to situation, is seen as analogous to the application of physical theory by the engineer, and it is an image apparently still widely held. Despite the prevalence of this be-

lief, a recent survey of anthropology and social science in industry and business says of its principles that "... all this suggests that at a minimum the practitioner who wants to apply human relations research has no clear directive as to what to do." (Wilensky, 1957). However, this notion of "experiment," "control" and "prediction" seems to be a goal of those anthropologists who work chiefly in the special action structures and part societies found in our own culture in which the power situation is clearly hierarchial.

The middle range of ideas about the relation of knowledge to practice, as well as the humanistic end of the continuum, comes mainly from anthropologists whose experiences have been cross-cultural, with whole societies and cultures, and where power relations and ends to be sought have typically been more diffuse or less easily grasped. In this area are to be found most of the British social anthropologists who have been in colonial situations. Mair (1957), in her short and penetrating review of applied anthropology, disavows the social engineering hope and image. She writes that anthropological principles are applicable but never experimentally provable. Raymond Firth (1958) has often expressed an idea of applied anthropology as the "specification of the alternatives and implications" of carrying out some plan or program. Here we see principles and practice related not in terms of control and prediction but rather diagnosis and prescription. This view has been refined and put to use, almost inadvertently, by a student of Firth's, James Spillius (1957) in Tikopia, and given the name of "operations research." In this middle range are applied efforts like Holmberg's (1958) experience in Vicos, Peru, trying to effect the transfer of power from *patron* to peasant, or the how-to-do-it manual of Spicer (1952) for the promotion of technical change while minimizing human costs.

Sol Tax (1952) and his associates on the Fox Indian project at Tama so define the relation of knowledge to practice that a new name is needed and they suggest "action anthropology" for it. They do not conceive it possible to merely apply knowledge to a problem situation, or even to say with much confidence what the implications of a plan or program may be. Instead, their course of action or intervention in a given case is the means

whereby principles and facts are to be created, rather than applied.

I do no more here than raise the question of the relation of knowledge to practice, but I shall return to it subsequently. It is relevant to recall the large body of opinion in contemporary social anthropology (Evans-Pritchard 1952, Herskovits 1955, Leighton 1945, and Redfield 1953) which maintains that whatever utility inheres in knowledge,—and the utility aspect of knowledge is not a chief concern,—is that of “understanding,” “insight,” “perspective” and other expressions which remind us that social anthropology may provide the basis for informed and humane judgments.

With this introduction, I wish to turn to a perfunctory summary of the contributions of the applied and action research of recent years to the understanding of man and to the image of social knowledge about man. My remarks will be cast under three broad rubrics: contributions to fact and theory; to method and technique; and to philosophy of the study of man and the image of man. Without burdening you with long citations, my discussion springs chiefly from the summaries of applied anthropology to be found in Kroeber et al., *Anthropology Today, Some Uses of Anthropology: Theoretical and Applied*; Mair's *Studies in Applied Anthropology* and the *Values in Action Symposium*.

Applied and action anthropology have over the years gathered an impressive body of data on peoples in colonial situations, on reservations, in factories, in hospitals, on haciendas, in trust territories, labor unions, and other formal organizations. The first effect of scanning the literature of those who write of applied and action anthropology is an appreciation of the extension of the situations in which anthropologists now gather information. Social anthropology as a whole is continually expanding its phenomenally relevant research areas, and the applied and action anthropologists have been in the forefront of this movement.

The fact generated by applied anthropology, on the whole, does not appear to be different from the ethnographic report of those not doing practical or applied anthropology. There are some notable exceptions as we move toward the action end of continuum. There, the reported fact tends to be more concerned with “mood,” with social-psychological item, with finer slices of

social and cultural life than most anthropologists usually tend to report. Again, social anthropology as a whole has been seeking to make ever finer and subtler descriptions and some segments of action and applied anthropology have been in the forefront of this current.

What is striking is the concerted insistence on specificity in the fact gathered. Especially have workers in action anthropology, where their facts carry consequences for a people's well-being, been insistent upon coherent, detailed, and weighted fact in their reports. These trends indicate a renewed concern with method as well as fact.

Contributions to theory (and here I mean no more than com-pendient empirical propositions) from applied and action anthropology represent no singular achievement (Redfield, 1956). These studies are, almost universally, concerned with social and cultural change, and they have highlighted for all anthropologists the virtual absence of a theory of change which is relevant to the micro-time of daily living. As anthropology as a whole moves toward theoretical questions involving cultural change, the dimension of "move by move rearrangements" in social life, which are the concern of applied and action anthropologists, becomes increasingly a general concern. It is too soon to ask what is the substance of concept and generalization which applied or action anthropology offers us about change. The Fox program amassed in its action program some attractive ideas about cultural change, but they have not yet synthesized them so that the experience with the Fox is transposable for other situations. Holmberg (1956 ms) likewise offers implications for a theory of social change, but the net increment in a general sense is small.

However, the effect of action, operational, and applied research on the kind of theory to be developed, rather than the actual development of theory, bulks larger. Action and applied anthropology stress the paramount fact of social decision making, of learning in new situations, and the multiple alternatives open to people in the face of new opportunity. Contrast this idea of what a theory of social and cultural change ought to be in more traditional, academic, or theoretical anthropology. Theoretically, we have usually favored social and cultural types, patterns, or dominant value profiles or some general feature of a social sys-

tem at a level of rather high abstraction (SSRC, 1954). There is a tension between something like Steward's (1955) idea of social and cultural typologies, or Nadel's "general law" (1956) in symbolic logic, and the indeterminancy, the pliancy, and the emphasis on choice, emergents, and unforeseeables stressed by actionists and some applied anthropologists. The tension is not irreducible, nor is the difference irreconcilable. But the discourses of action and applied anthropologists on the one hand, and of the theoretical, academic anthropologists on the other, tend to be at loggerheads over the kind of theory which anthropology as a whole needs.

This is a healthy antithesis and has caused us all to make clearer our criteria for good theory, the role of theory, and the relation between theory and action. Does a person committed to bringing about one or another social effect need a theory? How can the fact and finding of action and applied anthropology be fed back into the common storehouse of anthropological knowledge? How general should a theory strive to become when action and application are to be derived from it? These are typical questions agitating discussion about practical anthropology.

I give a personal and provisional, partial reply to some of them. Action people do not seek a theory; they are content to see themselves much like doctors of medicine. They practice on the social patient basing that practice on several bodies of knowledge, but without great emphasis on a theory of the whole, just as doctors treat the sick without an integrated theory of the human body, or draw upon knowledge based on different kinds of theories (i.e., chemistry as against psychology.)

Every body needs knowledge to act, and presumably the more knowledge, the more efficacious and less costly is the action taken, but most situations in which one acts are not covered by a wholly relevant theory, and may never be so covered. Actionists, in the familiar polarization about the role of theory to knowledge, lean heavily toward pragmatics, while academics by training and profession are pulled toward fashioning logically neat and elegant theories. Redfield (1958) speaks for the "great tradition" of Western science when he asks Tax to explicate further on a position of the action anthropologists which appears to bypass

a logical problem because there was no practical need to get an answer in terms of proceeding with a program.

Our scientific tradition tells us that theory, and theory of high generality, is the goal of empirical research. We want theories for two chief reasons (besides the cultural insistence on them): to build devices and to influence conduct. A third reason for theories is more technical and academic: we seek economy of presentation, and we seek relations and contradictions in propositions by means of an articulated, aesthetic structure of theory. Now actionists and applied persons do not think a theory or theories which can serve as a guideline for intervention or social action is likely to be the same animal (or species) as the theory which is logical, refined, and a highly abstract structure aimed at by most anthropologists.

So actionists and applied persons think of theory as a partial goal, not the end product itself. A theory bears consequences, and it is the consequences that interest men of action. Practical anthropology, I feel, will not be the workshop of theory construction. The role of applied and action anthropology is to keep our theories in some meaningful relation to human action; to insist on reality in hypothesis; to specify the range of indeterminancy in our formulations; and to continually uncover and describe situations which indicate the shortcomings and inadequacies of current theory. This is no mean role to assign to the action segment of anthropology. And it provides the most useful kind of feedback into the discipline I can think of.

To move from fact and theory to method is to see action and applied anthropology making its greatest innovations. Whether the method is "clinical" as Sol Tax describes it, or "operational" as Spillius calls it, or "research and development" as Holmberg speaks of it, there is a new dimension added to anthropological field method. (I leave out the quasi-experimental procedures developed by some applied researchers inclined to small group dynamics, on the grounds which Fortes named: billiard-ball sociology is not conceptually able to be meshed with anthropological studies of natural groups with historically derived cultures and role activities which count heavily in life experience.) The new dimension consists in the actionists' or applied anthropologists' ability to revise formulations in the face of effects based on

those very formulations. Actionists closely approach the medical image of work in this respect. Diagnosis, treatment, result; diagnosis, treatment, result; diagnosis, treatment, cure—a chain of provisional generalizations based on general knowledge, but paying exquisite attention to the particular case, until the given problem is resolved.

Methodological innovation in applied as against action or operations research has taken the turn toward greater refinement of standard technique, and as Mead notes (1956 a), it is not much of a departure. But the chief idea I find in the methodology of action research is the formulation of hypothesis which can be confirmed or discarded in a relatively short time and with relative confidence as to the correctness of the decision to accept or reject. How is this possible in action research and less possible in research ordinarily conceived and executed? The key lies in the mode of "intervention" (Peattie, 1958).

The basis for drawing conclusions about the genesis of a given social action and its consequent effects is that the action researcher is likely to be more fully aware than the usual investigator is of the premise upon which the action is predicated. Now, all social anthropologists deal with purposive social action, but they do not usually try to induce, or direct or participate in a people's working out the premises for making a decision. Action anthropology engages in the deliberate attempt to make explicit to the researcher and to the community he is studying, the grounds for doing something. Discussion about what is involved in a course of social action with the people about to take that action, seems distinctive of action research.

Consequently, an action researcher gets, in the ideal instance, a much clearer idea of why a thing is or is not done, of what the connections between parts of social life are, or of what a given people truly value. Action anthropology offers a way of describing a culture and a way of finding the rightness of that description which is rich in promise and provocative in implication.

Two questions arise however as to the limits imposed by the methodology—must research be limited to those things a community takes as a problem and does the method restrict findings to micro-time and micro-ethnography? I can offer no answer to these, for only time and action anthropologists will make relevant replies.

The method of action anthropology, and to a lesser extent the methods of applied anthropology, raise problems not only of scientific or intellectual dimensions, but also of moral and philosophical aspect as well. If one is methodologically engaged in finding knowledge relevant to a course of action, and if the course of action is seen as being based upon some ranking of preferences, and if one tries to clarify the reasons for these preferences, then one is committed to a moral, normative, and value element in the course of investigation.

Questions as to whose values or which values are to be regnant; questions as to the relation of knowledge to value; and questions as to the personal, emotional, and psychological burdens and joys of the research, immediately become pressing. For an applied anthropologist, these questions are not so burning, not so nearly at the forefront of his attention and discussion. Either an applied anthropologist subscribes to the values of his employer or he does not accept the employment. An anthropologist hired to serve as advisor to the trust territories either believes the administration seeks the natives' good in terms compatible with the anthropologist's personal ideas of that good, or he does not take the post. The applied anthropologist in this situation may not make policy. Nevertheless, the policy should be one to which he can subscribe, and in the framing of which his findings are an element. He may resign in frustration when he finds that the relation of knowledge to power clearly makes most of his research irrelevant. He may become irked by the administrator's or politician's demands that nuance, technicality, qualification, and concept be bleached from his report in favor of "the hard facts on which a man can act." Again, he can resign and go back to his university.

I purposely make dramatic this more complex polarity of being an employed anthropologist in a political or power content rather than in a research situation. I see no alternative for this kind of applied anthropologist, when the inevitable questions of value arise, but to resign, to revolt, or to accede to a value system which comes, so to speak, prefabricated with the job. Some jobs one can find morally tenable, others not, but the constraint is always that of the employing agency and its needs for policy based on fact toward ends it ultimately defines. An-

thropologists will continue, and in ever increasing numbers I believe, to take non-academic employment because of their special knowledge about non-Western peoples, but they always will face the dilemma of working in a situation where their results or expertness may not be highly valued, and where their reaction to policy, to ends, to the morality of the situation is equivocal and may lead to personal moral conflicts.

Action or operational anthropologists do not consider themselves to be primarily employed by an agency or institution, one part of which wishes to bring about some social effects on another part. The actionists or operational anthropologists are the self-committed; they choose to make values and activities toward some "good" a part of their anthropology. Such a choice can only be meaningfully made when the anthropologist is not employed in the same sense as the applied anthropologist is. Typically the action anthropologist is part of an academic community, or a community consultant. Such a role allows him to be guided by an ethic not dictated by the conditions of employment.

What is the value premise or premises which guide action and operational anthropology? It is clear that action and operational anthropologists feel themselves to be part of a world in which outside societies or power structures are attempting to intervene in the lives of the communities and cultures they ordinarily serve and study. They have come to do value laden anthropology through an emotional and heartfelt attachment to the people they came to study. They have stayed to make the community both subject and object of research. So the guiding values of action anthropologists tend to be those of "self-determination" for the people they study, or "human dignity" for the communities in which they are involved. These values come to be dominant in situations where small communities face larger, more powerful ones.

These are the broad and general values which induce actionists to avoid pressure, force, manipulation, deceit, propaganda, or other devices and dangers which may inhere in the attempt of bringing about social effects through deliberate means. The values expressed by action anthropologists are not very different from those most anthropologists would subscribe to, and probably

most people enculturated in a Western society. Actionists differ from most anthropologists in the explicit inclusion of values as a premise of research. In a situation like that faced by Spillius in Tikopia,—a crisis involving human life occasioned by crop failure,—probably, as Redfield (1958) notes, no anthropologist would have behaved differently, namely, helping in every way he knew how. But Spillius elevates help and aid to *the* job to be performed, and knowledge and fact are relevant only insofar as they advance this goal.

Similarly Sol Tax (1958) and associates see values as part of their scientific work. They feel that subscription to the duty and obligation to help a people while studying them is *the way* to arrive at meaningful statement of fact and provocative, fruitful hypothesis. From a look at the forthcoming *Reader in Action Anthropology*, it is clear that a value premise, like that subscribed to by action anthropologists, appears not to result in facts that are false or doubtful, or in hypotheses that are sterile. Lynd's (1939) old contention,—that the danger of letting values swamp fact and theory in social science is not great, and probably only exists when social scientists are in the service of some group seeking to hold or gain power,—is reinforced by the experience and work of the action anthropologists.

Whatever, then, other anthropologists take to be the values guiding their work, we are forced to reconsider the implications of different relations between science and values, when science is considered as a research activity. Problems may be selected because of their importance in somebody's everyday life; fact may be gathered to help and aid human beings without distortion of such fact, and values may be the object of research as well as its subject—these things contemporary action research tends to underwrite, to bring to general anthropological consideration.

Whether or not new field experience will cause us to revise these conclusions time will say. But I recently had a field experience which raises some of the problems about values in research activity, and which casts some light upon the position of action and operational anthropology, although I am not a practitioner of either.

In a pottery-making community in southern Mexico a group of Tzeltal Indians uses the simplest technological methods of

hand-forming and open-firing for their ceramic ware. The work of firing is hard and heavy, and the wood used as fuel is slowly destroying the forests near the community. The Mexican Government is interested in preserving the resources of the region and at the same time raising the level of living of the Indians. There is an National Indian Institute for the Tzeltal-Tzotzil communities engaged in economic development with Indians, health and medical programs, literacy and education, road-building and general improvement. The National Institute is ethically committed to helping the Indians on their own terms—they view the situation like action anthropologists, a free choice of alternatives presented to the Indians, who in the end know best. The National Institute suggested the possibility of introducing an adobe kiln to the pottery community. Since I was then in the field I undertook to ascertain whether or not the people of the community wanted a kiln. My research took me through the usual channels of understanding by traditional anthropological methods the economics of pottery-making, the social relations of work groups, the structure and pattern of community life, insofar as I could get data on this in the short field time then available to me. I then sought out some of my friends among the potters and asked them about kilns. I took pains to explain what the kiln was, how it would work, and the fact that they would bear no risk or cost in trying it if they were so inclined. I told them that I foresaw possibility of more income with less effort and even innovations in pottery-making like glazing which would expand their markets. I tried to foresee what inadvertent effects might ensue and make those clear to the best of my ability.

But what I learned most from this experience was that it was partly from my friends' implicit trust in me and my wife as persons that the willingness to try the kiln arose. I realized that if the kiln is introduced (it has not yet been made available to this community) we had a real responsibility in seeing the people through their adoption or rejection of it, and that a series of events beyond our scientific ken would certainly be initiated and that we again had the moral responsibility to help in the solution of the new problems and consequences of kiln-fired pottery. I also became aware that I took a hand in the affair of the kiln

because my value system places emphasis on enough food, clothing, medicine and education to meet a people's aspiration, and that I favor expanding leisure and making work less physically demanding. The Indians share enough of these values that they too were willing to try the kiln. I do not think my interest in the kiln or my value system will distort my reporting and interpretation, nor that of my wife or the graduate student who will be in the field with us for the coming session. At least it will have no more disturbing effect on my observation than the giving of medicine, legal advice, or taking an Indian's part in a dispute has had in previous field work. But I am not sure how much responsibility I can bear in regard to inducing change or being a party to the clarification of alternatives in a society in which I come and go as a research worker. In my own society, like other anthropologists, I do not hesitate to state my preferences to influence action, to solve problems, but there I bear full consequences, and citizen and anthropologist are not so easily kept apart.

The discussion of values in science and in action and applied anthropology cannot be left without a more general consideration of the role of knowledge in the formation and holding of values, as apart from the function of values in generating and subscribing to knowledge. It is generally true that a person's or a community's system of normative propositions bears a determinate relation to the body of knowledge and technique available. I do not mean that values spring from fact or are determined by fact, but that our notions of what ought to be are in part determined or influenced by our notions of what is and what may be in the phenomenal world. What we know, or think we know, about man and culture influences what we think man and culture ought to be. So even though values come from the consensual meshing of a community's preferences, that consensus is a condition vitally influenced by the store of knowledge. All knowledge has some implication for behavior and conduct and thus for values. And as anthropologists begin to make values both subject and research tool, a kind of knowledge is fashioned which expands the universe of discourse about the aims of social and cultural life, about the images of the good society, about the identification of the good man, and about the measure of the well spent life. Our anthropological knowledge becomes part of

the philosophy of man, and perhaps of discussion as on how to achieve the states we take to be desirable. All this is implicit in the study of society and culture, for however we try to make our knowledge neutral (and I do not know anyone who tries very much) it is not possible to make true statements about man in society which do not have the potentiality of influencing value positions. Society is not like a rock formation; purpose and human need lie at the very center of the phenomena themselves. Values, as Nadel (1956) puts it, are entailed in the very study of society and culture.

From applied and action anthropology we have learned some important and significant truths, that were not perhaps so forcefully borne home by theoretical or academic anthropology. The chief gain, I should say, is the better understanding of the nature of human action in a social and cultural setting. We are firmly convinced, both from the successes and failures of action and applied anthropology, that organized social life can not be manipulated in any sense. Social life sets problems for people and they work them out according to the constitution of their society and culture, not on the basis of the "best" or most "logical" or "efficient" solutions. In fact, those who have tried to aid in inducing or directing or mitigating the effects of social change, have come to the conclusion that what a people does, the solution they reach, is "best, logical, and efficient."

We have learned the pleasing truth, that society talks back. Even the small-scale, technologically inferior peoples of the world have tremendous powers to resist change they do not want, and to adhere, often at great cost, to their valued and distinct way of life. At the same time we have learned that changes which people desire, radical or not, can be made swiftly, without great cost, and that a society may nearly re-do itself—in a generation if it wants to (Mead 1956b).

Action and applied anthropology then, seem to me to serve as extensions of situations in which we gather facts, as a place for the refinement of method and hypothesis, and as the continual generator of pressures for the discussion of ethical positions in anthropology.

As Lisa Peattie has said, applied work will continue to raise problems of ethics, of social and cultural change, and of power

relations between societies. This range of problem is certainly the range in which anthropology as a whole is operating, and its applied and action segment may open new vistas.

Action and applied anthropology is the use of knowledge in the situations of everyday life, and this trend is apparently one of growing strength. As this grows we are forcefully reminded that everyday life is not only, or even chiefly, a scientific problem, but an arena calling on all the diverse capabilities of man. Certainly action and applied anthropology bring us to a closer understanding of those capabilities.

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FIELD WORK: THE COLLECTION AND ANALYSIS OF DATA

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Prior to my first field work, I picked up the information that one needed stoutly sewn, hard-covered notebooks, a large supply of well-sharpened pencils (ink might run if one were caught in the rain, and it was before the pen that writes under water was developed), a minimal grounding in linguistics, and sharp eyes and ears. It was also suggested that a camera might be handy, a supply of money useful, and a knowledge of what had been previously reported about the group with which I was to work essential. Thus equipped, and thoroughly aware of that most important ingredient of successful field workers, the ability to gain "rapport," I set off with my wife into "the field."

Granted that this was not the most desirable preparation from some points of view. I had a problem—to collect folk-tales. I had a general background in Northwest Coast Indian culture, and had read all available material on my specific group. My wife was working on another problem, one quite unrelated to folklore. We arrived at Neah Bay, Washington, with fear, trepidation, and a sense of adventure (despite the fact that Neah Bay was by no means isolated from "Whites," movies, logging camps and the Coast Guard). I admit that the sense of adventure has not yet left us when we contemplate field work, wherever it may be. Given these feelings, this background, these problems, how does one start to collect the data? Perhaps we were incredibly naive—in terms of modern "research design" we were absurdly unprepared—but we proceeded to find a place to live, and we took walks. We had some names of potential informants, and we gradually made contact with them, or they with us, after almost two or three weeks of being seen about town. To the casual observer, we must have looked like aimless vacationers. To ourselves, we appeared to be engaged in vital activity—examining driftwood and old canoes on the beach, looking under houses, hunting for landmarks long vanished that had been mentioned in works on Neah Bay. We noted everything in our journal that struck us as strange, or commonplace, or interesting. To this day,

we cannot say when or how we established rapport—either with the village or with our informants—but within a month and a half, plus or minus a week, we felt we had it. In fact, looking at our old journal and notebooks it was only toward the last month of our three-and-a-half month stay that we began to work intensively with informants. Yet we already had gathered a good deal of material strictly by observation.

I have given you a brief glimpse of this first field trip because I wanted to emphasize the "do-it-yourself" nature of my field training. It is because of the way in which my wife and I entered the field, and the things we learned from it, that I am reluctant to generalize about the collection of data in the field. Certainly one can use the standard rubrics—problem-centered research, genealogical method, rapport, participant observation, etc.—to describe the activities of almost any anthropologist in the field. But each field situation is new, both to the field worker and to the people with whom he works. Whatever his interests or approach, the field worker has a single goal in being in the field. That goal is the acquisition of certain kinds of information, data if you like, which he feels it is necessary to acquire. Working with specific people, under specific circumstances, and usually for a limited period of time, any technique which will bring forth the desired information is a good technique. One might say that the watchword of the field worker is "adaptability."

It is the practical problems of field work which raise major questions about anthropological data in the minds of non-anthropologists (and some current anthropologists as well). How can one be sure of the comparability of data if the techniques of collection are not standardized? If the field anthropologist finds that data of the sort called for in his initial field proposal are not forthcoming, if he returns with different kinds of material, has he been derelict in his duty, poorly trained, incompetent, or simply very acute in his perception of the situation? When the field worker records actions, ideas, beliefs, patterns of behavior, is he recording something real, "factual," or is he reading into observation his own conceptions? How involved in his data is the anthropologist himself?

These are obviously questions with a philosophical overtone. In a sense they are questions which any anthropologist asks himself during the process of collection and analysis of his data and

of evaluation of the work of other anthropologists who have been connected with "his people." If the field worker fails to perceive what another field worker in the same culture has reported, is it the fault of the anthropologist, or is it a result of the situation having changed? For example, can one attribute the disparity between the reports of Goodenough and Fischer on Truk residence patterns within a three-year period to the inaccurate observations of one or the other, or to drastic change in so short a period, or to differing orientations affecting the two field workers' investigations?

Let me return to my first field trip once more. I entered the field with a problem—what anthropologist hasn't? But I had had impressed upon me the necessity to collect everything which might be of value in examining my "problem" later. In the back of my mind was the thought that I need not really collect "everything" in the standard ethnographic categories. There were other investigators of my culture from earlier periods who had "done" various segments of the culture. Besides, I told myself, there is reasonable doubt about the ability of even the most capable field worker to collect data of the kind demanded by the standard ethnographic categories: material culture, social organization, economic organization, political organization, religion, art, folklore, dance, personality (or individual life cycle), language, etc. I could not hold myself personally responsible for data in all these spheres of activity. Armed mentally thus, I concentrated mainly upon folklore. But my wife collected data about diet, and incidentally we did something on marriage and genealogical data. One informant wanted to talk about basketry as well as folklore, and another about fishing practices. When I glance now at my journal I find it has recorded material on these items plus residence patterns, village plan, council meetings, attitudes toward government, linguistic data, comments by informants about rank and class, physical training of youth in the old days and at present, etc. My major collection was folklore—but I realize now that with a better realization of my position as primary data collector at a particular point in time I failed to pursue in depth many of these items.

It is not enough to say that the outlines of the culture are probably well recorded by others. Certainly for Neah Bay this

is true enough. The patterns which were unique to Neah Bay in the Northwest Coast Culture Area are extractable from data going back to the 1600's A. D. But, from another standpoint, my "problem" orientation obscured an important point—i.e., that the Neah Bay of 1950 is not the Neah Bay of even the 1930's nor of 1958. A more minute observation and record of *all* aspects of Neah Bay life in 1950 would have been immensely more valuable to the culture history of the Northwest Coast area as a whole and with culture processes in general, than is a good and detailed collection of folklore, supplemented by scraps of other data. One might say, "Well, that but indicates that you were not yet an experienced field worker." I feel, however, that it is more indicative of a kind of field work which concentrates so intensely upon the problem at hand—be it kinship systems, economy, political patterns—that it forgets a primary purpose of field work. This purpose I now see as being the traditional one of collecting as complete a picture of the culture at a given moment, as it is possible to do within the bounds of one's time. To be sure, we will probably be deficient in certain categories, depending upon the focus of our interests. But we will not be as deficient as we are when we consciously exclude certain areas of culture either because "someone has recorded them for the past," or because they are not pertinent to the problem at hand. Perhaps my own experience has made me overly critical of those anthropologists who argue that enough data of the ethnographic sort has been collected, so that we can begin to refine our collection and foci of interests. I shy away from the argument that we can, or should, single-mindedly pursue a problem in the field without regard to "less important" aspects of culture. I feel that when actually carried into practice, this technique of collection leads to a kind of data which is lacking in the essential ingredient of anthropological data—the ingredient of context. Without the context being given in the form of data, we are, in a sense, at the mercy of the anthropologist who is more intent upon generalization than upon delineation—that is, we are forced to evaluate data in terms of the anthropologist's conceptual scheme, problem, logic, etc., rather than in terms of its significance within the cultural framework. In some ways, I am bemoaning the passing of the more adequate ethnographic presentation of ethnographic material.

In some respects, it is more possible today to do a rounded job of data collection than it was in the past. Mechanical aids such as different kinds of cameras and films, tape recorders, soil analysis kits, distribution maps, and variations on these themes are available for supplementing the hard-covered notebook and the pencils. What is perhaps more important is that many peoples have been brought into contact with these devices who in a former period would have prohibited their use: today the camera may be an additional way of establishing rapport, rather than a device for stealing away souls. The field-worker who does not bring back pictures and broad ethnographic data may in fact be derelict in his duty, for here exists a prime technique for obtaining data on many facets of the culture without a great deal of effort—data which may be used with proper caution by non-field workers to gain some of the “flavor” of the field, as well as for further analysis of different problems. But here let me add that the interpretation of pictures again places an onus upon the field worker, since it is necessary for him to provide the background against which the pictures were taken—in fact he must at least write the captions before the pictures can be somewhat objectively used.

Again, I begin to creep over into analysis of the material before we have finished collecting it. And yet, is this not what happens in the field as well? When I was obtaining my folklore, I would often refer back to Frances Densmore’s collection of Neah Bay data, specifically to her few tales, to see what comparisons there might be, whether my informants were also checking her book (several copies existed in Neah Bay), and what variations on the theme I was obtaining. Sometimes consciously, sometimes unconsciously, I would take from the Densmore material a theme, a character, a minor point and use it to form a leading question to present to my informant. In current terms, I had a “model” against which I was working. If, however, the informant’s material did not match the “model,” I was not particularly concerned about the “model.” I had more interest in obtaining my data than in proving, disproving, or correcting someone else’s data. I think it is on this point that a division is sharpening in anthropological field work, a division which has not yet hardened into a distinct cleavage. It is sort of like the division that separates

those who read the end of a mystery story early in the game, and then go back to the beginning to see how it is developed (Type "A"), and those who try to figure out as they go along what the solution will be (Type "B"). One brings to a mystery story the accumulated background of mystery-story reading, one's familiarity with the culture within which mystery stories are usually set, and one's powers of observation. If one is the "Type B" mystery-story-reader, the synopsis and the setting (discounting the cover pictures) set one to forming a hypothesis. As more data accumulate, the hypothesis may be discarded, revised, confirmed. When done, one usually finds that the author has hidden from you some essential piece of data, or has misdirected your attention to divert you. The field worker who is more concerned with absorbing data and is fascinated with the immediate picture he is building is in much the same state. The hypotheses developed in the field may be sound hypotheses, but he will not become wedded to one solution until he has had a chance to get in all the data, and to see whether his attention has been properly focused. The "Type A" mystery reader, however, starts out with a hypothesis, and is hard to divert. He has read the solution, so in reading the book he heeds only those specific lines of data which are leading to the denouement, drops those which seem irrelevant, ends up with the data which are selected as useful in terms of the particular solution — or hypothesis. The field worker who parallels this "Type A" in terms of data collection has made a decision before he enters the field. Only in field work, in culture, the last few pages have been torn out, the field worker has not been able to read the solution. Yet his pre-field decision may, if adhered to firmly, limit considerably the amount and type of data that goes into his notebook. I would argue that his periphery of observation is also thereby directed—that, in fact, he is less prone to collect material that does not seem pertinent at the time to his original hypothesis, but which well might have had considerable pertinence in the culture.

Now these two types are, of course, extremes. Most field workers, it seems to me, consistently do neither one kind of collecting nor the other, but combine both methods. I am tempted to state categorically that the way in which data are collected is a matter of personality, interests, state of health, receptivity of the

group being worked with, in short, the adaptability of the field worker to his field. In a sense, the degree of commitment to assumptions about the nature of culture and society, as well as to a particular problem, tend to shape the nature of the data collected and the techniques of collection. The field worker who is attempting to justify a particular technique or to convince his colleagues of the scientific nature of his method may well be more rigid in his approach to a field situation than one whose prime approach is a sense of the uniqueness of it all. If I sound suspicious of the assumed "objectivity" of some field workers, I admit to the suspicion. I am not yet sure that one can be truly objective about specific peoples or societies with whom one has worked. Perhaps the most objective field worker is one who ends up neither violently detesting nor violently approving of "his people."

I started out by noting the minimal equipment that a field worker carries into the field. I have indicated, by the trend of the discussion, that the field worker himself is a vital part of the field process—his interests, background, approach to problems, past experience. The field worker himself can be viewed as a complex piece of equipment—a recording instrument which does more than just record at random. In the last analysis, the adequacy of the data obtained is dependent upon the adequacy of the field worker. It is this intensely personal tool which shapes the nature of our data and the conditions of its collection. Add to this balance the involvement of the field worker with individuals belonging to the culture under investigation, problems of language, problems of perception, problems of selection, and one begins to wonder that there has developed any way of comparing material obtained by different workers. In one sense, one might say that the ability of diverse individuals to agree on the essential similarity of certain aspects of behavior observed in different cultural settings is at least minimal confirmation of the reality of such similarity. In this sense, some of the standard categories under which material is collected and reported have emerged from the data as much as they have been constructed *a priori*. The field worker on the whole, is distinguished from tourists, colonial officials, local inhabitants, by a sense of the ways in which "his" culture or people are like others, as well as by a

realization of when he encounters an aspect of their culture which is unique. In short, he knows what he is looking for, and recognizes it when he sees it. As a recording instrument, then, he is more finely adjusted than other recording instruments which do not differentiate or discriminate between the unique and the general.

If I have conveyed the heavy burden which the field worker carries, I have succeeded in what I set out to do. For I feel that it is at the field level that the anthropologist is made. It is in the field that one first begins to realize the reasons for some of the deficiencies or beauties of other anthropologists' field reports; it is here that one comes to grips with the "ethnographic present." Some anthropologists have argued that it is in the field that their whole orientation toward anthropology solidified. In any case, it is the actual, intimate, personal contact with a different culture, different habit patterns, different ways of perception, that has been one of the distinguishing marks of anthropological data collection. Through this personal contact, the anthropologist is sometimes able to assume the difficult role of one both apart from, and a part of, the culture under observation. If I were to stress any one aspect of the equipment a field worker needs, it would be acuity of observation, for the field worker frequently finds himself "seeing" patterns of behavior that are not consciously perceived by those with whom he is working. In a real sense, the anthropologist cannot help interpreting the statements, actions, structures, which he sees about him, despite any conscious attempt to be strictly "objective." It is because he is alien to the culture, is consciously trying to empathize with the culture or some of its representatives, that he is forced to interpret and re-interpret until it "clicks"—i.e., until his informants or people observing him discern elements in his behavior which coincide with their experience or feeling of what is "proper." In the attempt to become accepted, to practice, for example, a gesture until it looks and feels natural, the field worker begins to absorb a part of the culture, to make it enough a part of himself so that later, working with the data will enable him to recreate his notes into an approximation of reality.

What really separates the collection from the analysis of data? If the field worker hypothesizes, analyzes, speculates about his

material as he gathers it, if he compares it to material about other societies which he has known intimately or has studied, has he not already begun his analysis? Of course. But the problem of analysis does not stop there. The purposes of analysis, like the purposes of collection of data, are many — to recreate the broad outlines of the culture under study, to abstract patterns which may be compared to similar patterns in other cultural configurations, to throw light upon the relationship of local behavior patterns to local ecology, political forces, economic pressures, etc.—these are but a limited handful of the kinds of demands which might be placed upon the material collected in the field. Satisfaction of demands depends to a great extent upon how specifically the field trip was formulated in terms of problems, to some extent upon the interests of the field worker, and to some extent upon the accessibility of the material in the specific field situation. Unexpected relationships frequently are seen in the field, explored and then systematized after the return from the field in the light of further research. To attempt to chart the multitude of ways of analyzing material is as overwhelming a task as is the attempt to delineate specific ways of doing field work, beyond the basic “go forth and labor.” There are certain techniques of analysis which any (except the most systematic of anthropologists) must use upon returning from the field. The first step in analysis is to order the data which is in splendid disorder within the notebooks. It may be safe to say that even if a standard ethnography is not to be the end result, the first ordering is in the pattern of such ethnography: material culture, life cycle, economic patterns, etc. This step may be done either on paper or by “feel,” that is, notebooks may be read, reread, retyped—in general, the field situation relived, but relived in a different conceptual milieu. Having “reabsorbed” the material, the analysis then can proceed to the stage of selection—selection of all that seems pertinent to a particular problem. This stage may call for further research into library sources, presentation of the material in short papers which elicit comparative data, discussion with colleagues, or perhaps (though less and less frequently) publication of an ethnographic report. How much time elapses between the field experience and the first publication of data after some sort of analysis? It would be a safe

guess that at least a year or two is the minimum period involved. Not many of us are fortunate enough to be able to continue to live with our material continuously from field through publication. Perhaps, however, this interval should be considered an essential step in the analysis—perhaps it is in this period that one can see the data in a new light, discover (subconsciously or subliminally) suggestive nuances in it, place it in juxtaposition to previously unthought of data. I would not carry this argument too far, but perhaps we are able to gain a distance from our field which is emotionally necessary to evaluate it with some objectivity.

I have not spoken of techniques of analysis as much as stages in the process of analysis. This is intentional. Techniques which are suited to a problem of economic anthropology may have little pertinence for a problem in child rearing. The extent to which we are able or willing to generalize from the specific data to more abstract problems determines our choice of technique as much as the adequacy of the material itself. The bias of the worker toward or against specific conceptual schemes, the tendency toward a broadly "humanistic" or "social science" or other approach, all affect our analysis. In general, the prime responsibility of the field worker is to present his data in such a way that it does justice to his informants and their culture; secondly, to present it so that it is understandable by his colleagues in the discipline, and so that it may provide further "primary" source material for subsequent research; and finally, to present it as fully as possible, together with a specification of the conditions under which the data were collected, the limitations as seen by the field worker, and suggestions as to gaps as perceived by the collector after working with the data. These ways of presentation of the data are not always mutually inclusive—in fact, the last method of presentation is one not often possible. Yet it is the last method of presentation which might lead to a more precise formulation of field method and to more use of anthropological data by scholars in other disciplines.

I have deliberately avoided using the term, *scientific*, as much as possible in the previous discussion. The problems of making our techniques of collection and analysis of data more precise, more "scientific" are well-discussed in such compilations as *An-*

thropology Today and *Current Anthropology*, and regularly appear in the journals such as *Human Organization* and the *American Anthropologist*. There are, as is well known, many currents in anthropology, and many divergent points of view in regard to the desirability of the discipline becoming more "scientific." These divergences are reflected in the problems selected for investigation in the field, the techniques of analysis of data, the presentation of the data. New problems are faced by, and new techniques are demanded of, the anthropologist as he moves into the area of complex cultures. It is, in the last analysis, the combination of participation, observation, informant-probing and absorption which defines the basic field method. And it is the fact of possessing, in fact of controlling, as a result of this method, unique and truly primary source material that shapes the earliest stages of analysis. Whatever may be done with the data beyond this point is subject to the individual interests of the investigator and the state of the discipline at a given moment in time. As long as change is a constant element in human culture, the "field" will be new. As the history of specific cultures grows with each field worker's reports, new problems will send other workers to the same field. However many technical aids are developed, it is the individual field worker who perceives, participates and interprets the data. Until we can standardize people, we can look forward to variation in the collection and analysis of anthropological data.

ACKNOWLEDGMENT

The Institute on Social Anthropology, Saint Louis University, June 9-14, 1958, was organized and directed by Dr. Allen Spitzer, Associate Professor and Director of Anthropological Research, Saint Louis University. Other members of the Faculty of Saint Louis University who participated as Staff of the Institute are: Rev. Robert J. Henle, S.J., Academic Vice President and Dean of the Graduate School; Rev. Martin F. Hasting, S.J., Dean of the College of Arts and Sciences; Dr. Clement S. Mihanovich, Director, Department of Sociology and Anthropology; Rev. Trafford P. Maher, S.J., Director, Department of Education; Dr. William C. Korfmacher, Director, Department of Classical Languages; Dr. John W. Conover, Associate Professor of Geography; Dr. John J. O'Brien, Associate Professor of Education; Dr. Marilyn K. Rigby, Assistant Professor of Psychology; Dr. Joan de Pena, Instructor of Anthropology.

Guest members of the Staff of the Institute were: Sister Inez Hilger, O.S.B., St. Cloud, Minn.; Dr. Irving Kaplan, Assistant Professor of Anthropology, Washington University; Dr. Gottfried O. Lang, Associate Professor of Anthropology, The Catholic University of America; Dr. Robert J. Miller, Assistant Professor of Anthropology, and Dr. Beatrice Miller, Washington University; Dr. Manning Nash, Assistant Professor of Anthropology, University of Chicago; Dr. Robert Redfield, Robert Maynard Hutchins Distinguished Service Professor of Anthropology, University of Chicago.

The publication of this special issue of the *ANTHROPOLOGICAL QUARTERLY* has been made possible through the cooperation of Saint Louis University, not only in making available the papers of the Institute on Social Anthropology but also in giving a substantial subsidy for this number. The University is expanding its offerings in Anthropology and now gives the M.A. in that field and the Ph.D. with Sociology and Anthropology in combination. This is welcome evidence of growing support of the field of Anthropology under Catholic auspices.



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